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Proceedings of Symposium on Structural Dynamics under High Impulse Loading

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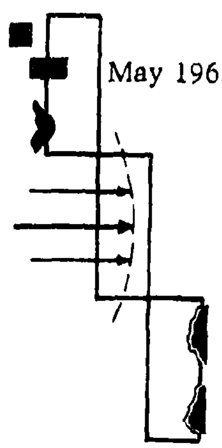

May 1963

Flight Dynamics Laboratory
Deputy For Technology
Aeronautical Systems Division
Air Force Systems Command
Wright-Patterson Air Force Base, Ohio

Project No. 6906, Task No. 690601

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Aeronautical Systems Division, Dir/Aeromechanics, Flight Dynamics Lab, Wright-Patterson AFB, Ohio.
Rpt No. ASD-TDR-63-140, SYMPOSIUM ON STRUCTURAL DYNAMICS UNDER HIGH IMPULSE LOADING. May 63. 423p. incl illus., tables, refs.

Unclassified Report

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"Mechanical Properties of Solids"
"Wave Propagation Phenomena and Structural Response"
"Hypervelocity Impact"
"Fracture Phenomena"

(over)

1. Dynamics of Structures
2. Stress Wave Propagation
3. Hypervelocity Impact
4. Material Properties
I. AFSC Project 6906, Task 690601
II. Avai fr OTS
III. In ASTIA collection

The session presentations point out the problems which exist in the development of reliable aerospace vehicle structures. These structures, orbital and otherwise, may be subjected to violent impulsive loads. These loads may come from collision with meteoroids, man made pellets or other energy sources. In each case the loads are of enormous magnitude and extremely short duration. They are several orders of magnitude more intense than loads experienced by structures in the familiar earth bound environment. Continued operation of space vehicles subjected to such hazards, must be insured by intelligent design.

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FOREWORD

The USAF Symposium on Structural Dynamics Under High Impulse Loading was held by Aeronautical Systems Division and The Office of Aerospace Research at the Biltmore Hotel, Dayton, Ohio, 17 - 18 September 1962. The purpose of this symposium was (1) to establish the fundamental concept of a national program in the area and to emphasize its importance to the aerospace vehicle design, and (2) to inform management and technical personnel of industries, universities, nonprofit organizations and Government agencies of the requirements of this field.

The proceedings are set forth in this report in the same order of presentation that was followed during the course of the symposium. Open forum discussions were held following each presentation. Session chairmen and speakers answered questions originating from the floor. The impromptu questions and answers were recorded by a stenographer and are included at the end of the account of each session. The exchanges were edited by the Technical Editing Committee only to the extent necessary to present the material in a clear concise form.

During the course of the symposium it was announced that the proceedings would be published within thirty to sixty days. However three months elapsed before all the information was released by the authors.

If serious errors or omissions are noted in this publication, they should be brought to the attention of Mr. Francis J. Janik, Jr., ASRMDS-1, Wright-Patterson Air Force Base, Ohio. Necessary errata sheets will be prepared and distributed.

The success of this symposium was primarily due to the fine contributions of the speakers, session chairmen and authors of technical papers. The Symposium Committee gratefully acknowledges these contributions and expresses its appreciation. The Symposium Technical Committee likewise expresses its appreciation to the many who gave unselfishly of both their time and talent in making this symposium possible and successful.

ABSTRACT

The Symposium Proceedings on Structural Dynamics Under High Impulse Loading are presented in this report. The Proceedings are divided into four major technical areas which are as follows:

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PUBLICATION REVIEW

FOR THE COMMANDER:

RICHARD F. HOENER
Chief, Structures Branch
Flight Dynamics Laboratory

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by

L. R. Standifer, Colonel, USAF
Deputy Chairman of Symposium

Director of Directorate of Materials and Processes
Aeronautical Systems Division
Wright-Patterson Air Force Base, Ohio

COLONEL L. R. STANDIFER

Deputy Chairman

I am Colonel Standifer, Director of the Directorate of Materials and Processes, Aeronautical Systems Division (ASD), and the Deputy Chairman for the symposium. I'm particularly interested in this symposium, sponsored by the Flight Dynamics Laboratory, Directorate of Aeromechanics, ASD, and the Office of Air Research, in that a great deal of it is the materials aspect of the problem of fracture and impact loading. I feel that a combined and energetic effort among the design people, the classical mechanical people, and the materials people have to be emphasized before we can live with some of the materials that we are coming up with in the materials area. The fact is, I see at the present time little hope of being able to make materials that will meet the average aerospace designer's requirements unless he learns to design and recognize, by his definition, that they are brittle, although by ours they may have some ductility. One, two and three percent ductility, we may think, is good in things like structural beryllium. We can beat this in some cases; but when you are going to coat the material, and do the other things necessary to protect it from its environment, someone is going to have to learn to design with the considerations of brittleness in the materials in mind, particularly if you want the strength at high temperatures. Actually, the high temperatures don't appear to bother us too much, but getting up there and getting back down to room temperatures again is a big problem. I say that it is of vital interest to the materials people as well as the mechanics and the design people.

The chairman of your symposium today is Major General Ruegg, the Commanding General of ASD. In the past he has been a laboratory chief, and speaking of the laboratories, they are quite happy to have a gentleman that has so-called "come up through the ranks of the laboratory" as their new commander; and with that, I would like to introduce the chairman of this symposium, General Ruegg, who will give the welcoming address.

WELCOMING ADDRESS

by

R. C. Ruegg, Major General, USAF
Chairman of Symposium

Commander, Aeronautical Systems Division
Wright-Patterson Air Force Base, Ohio

MAJOR GENERAL R. C. RUEGG

Chairman

Thank you. Lady and gentlemen, and we do have one lady back there taking notes. It gives me a great deal of pleasure to welcome you to this symposium on behalf of its co-sponsors, the Office of Aerospace Research and the Aeronautical Systems Division.

Our goals today and tomorrow are to establish a fundamental research program designed to enhance the state of science in solid dynamics, and to fill the need we have for advanced structural criteria for aerospace vehicles. These structures must be designed for both neutral and hostile environments. A fundamental knowledge in the areas which are under scrutiny at this meeting we hope will provide the basis for solution to our problem. The dynamic response of materials when subjected to very high forces of short duration is a relatively new field in this area, when compared to the overall science that we are all familiar with in our experience on structural problems.

New vehicle concepts require the development of increasingly accurate theory for predicting the effects of high impulse loads. The resultant structural technology is necessary to maintain the overall integrity of the vehicle and to provide the science for our future design advances in this area. These new vehicles will be required to resist the effects of hypervelocity impact, other impulse loads, and the attendant malfunctions that go with all our systems from time to time. So important is this aspect of aerospace power that we feel it is mutually valuable to survey this whole area with you. We devised this symposium to assemble the foremost people engaged in active research in the science of structural design. The purpose, I think, can be summed up in about four statements: (1) To present to you challenging new structural problems anticipated in the design of future aerospace vehicles; (2) To establish the present status of research in these areas; (3) To provide a basis for presenting bold new research objectives essential to advancing the state-of-the-art rapidly and effectively; and finally, to widely distribute the symposium proceedings that will result from this conference, delineating these research goals and problem areas for more effective utilization of the total technical resources at all levels.

You, a group of management and technical experts, have been assembled from the academic world, from the commercial world, and from our Government research organizations, to discuss how America will meet this challenge of science in our future aerospace vehicle design problems. As a space oriented nation, we must recognize that the solution to structural problems we face will cost a great deal--and the present costs will go up quite drastically in both time and energy, particularly in the fields of basic and applied research. The research accomplished is vital to the design of aerospace vehicles and we feel it is necessary, therefore, that these problems be resolved on a national scale to prevent unnecessary duplication of effort. Your enthusiastic response to our invitation to participate in this symposium was indeed gratifying, and I am particularly pleased to have here all of you people who are experts in your field.

Our speaker was to be Major General Marvin Demler, who is the Chief of our Research and Technology Division. Unfortunately, he was called to Washington and it seems that the priority and the call on the Hill takes precedence over most anything else. However, we do have a most appropriate substitute for General Demler, General Fred Ascani, who is not a stranger here by any manner of means. I am sure that most of you either know him or have met him through business dealings in the past. Among his many assignments at

Wright-Patterson, he was at one time Director of Laboratories and as such he had overall responsibility for all of our laboratory effort, so I think he is particularly well qualified to substitute for General Demler and I'm sure General Demler would feel gratified to have him fill in, so now I'll introduce General Ascani.

KEYNOTE ADDRESS

by

F. J. Ascani, Brigadier General, USAF

Deputy Commander, XB-70
Aeronautical Systems Division
Wright-Patterson Air Force Base, Ohio

BRIGADIER GENERAL FRED J. ASCANI

Keynote Address

General Ruegg, members of the sumposium, and guests. General Demler has asked me to express for him his disappointment in not being here this morning to deliver this keynote address. He does convey, however, his best wishes for a most productive and successful symposium. I, personally, am very pleased to participate in this Symposium on Structural Dynamics Under High Impulse Loading. The topics to be discussed, mechanics of solids, wave phenomena, hypervelocity impact, and fracture phenomena, are of great interest to the Air Force. They represent areas in which we see the need for significant advances before we can fully exploit the great potentials of aerospace systems. The importance of these areas in which the dynamics of the molecular and crystalline structure of the solid materials themselves become predominant was not particularly recognized by the Air Force until fairly recent times. Then our engineers were confronted with problems of hypervelocity impact and high altitude nuclear weapons effects. We very quickly became aware that our knowledge is quite limited on the behavior of solids under high impulse loading, the high temperatures and stresses which are developed, and on how dynamic fracture may occur.

I am pleased to note the progress that the research community has made in these fields in the past few years. A symposium of papers on the current topics could not have been assembled a surprisingly short time ago. However, I realize that the major portion of the recognized problems remains to be solved, if we are to reach our goal. The hoped for result of the current and future research in these areas is decreased vulnerability of future aerospace vehicles. The ability to survive the meteoroid and energetic particle environments of nature and the high energy impulses man may induce through nuclear activity is a prerequisite to reliable operation at will in space. We desire to attain materials and configurations which will provide the necessary protection without sacrifice of useful volume or weight. In research, where there is a constant interplay of theory and experiment, the greatest and most significant advances are made when the two progress together, each complementing the other. The task of the theorist who must develop the analytical techniques for these areas is most difficult. At least equally difficult is the role of the experimenter who must devise the experiments to obtain the necessary fundamental data for the theorist. Unfortunately, our present instrumentation is inadequate to the task, and this restricts our understanding of the phenomena we are investigating. In effect, this problem is a breakdown in communications between the investigator and his experiment. Perhaps through this symposium, some of us will find solutions which have been effective in other fields and develop new ideas which can be applied to our problems. Through the exchange of information about to take place here, we may gain an understanding of how to measure shock waves in materials, edge effects, cratering, fracture, and other phenomena of solid dynamics. This new knowledge, then, will enable us to corroborate present theories or disprove them. Serviceable measuring devices are not enough. Before we can adequately measure and evaluate our experiments, we must design our equipment to insure a high degree of reproducibility. It is absolutely mandatory to be able to isolate the one testing parameter we are varying, while holding all the others constant. The problem of generating extremely high pressures and temperatures with confidence in their reproducibility is both pressing and difficult. Solving the engineering problems of instrumentation and reproducibility of experiments will give us a great head start towards understanding some of our more basic theoretical problems. For example, "What really makes impulsive loading different from static loading?" "Why and by what mechanism do loads of impulsive origin induce internal structural changes?"

These, then, are some of the problems we are facing today. We know that other problems exist, problems whose nature at present is indiscernible. We hope that through this symposium we may not only search for solutions for the problems already mentioned, but bring to light and clearly state those that are before us.

The Air Force hopes to gain something more than the exchange of information from this symposium. We hope to gain enough insight concerning the problems that face us that we may aim our efforts toward the prime targets. Our national resources must be effectively applied in the area of survivability in space if we are to achieve freedom to operate in the natural and man-made hostile environments we may encounter there. We seek to enlist your help in planning a suitable research program in the area of high impulse loading effects. You are the research and management people most intimately concerned with the subject. We would like you to consider yourselves a planning group charged with the direction of a national program for the next five or ten years. To help you in the effort, the speakers will summarize the work done up to now, recapitulating the major advances and attempting to focus your attention on the remaining problems. Your participation in the panel discussions will not only give you the opportunity to state your views on the current state of affairs, but will also be vital in helping the Air Force establish a coherent program for the future. Thank you.

TECHNICAL SESSION I

MECHANICAL PROPERTIES OF SOLIDS

Daniel C. Drucker, Ph.D.
Session Chairman

Chairman, Physical Sciences Council
Brown University

TECHNICAL SESSION I

INTRODUCTORY REMARKS

COLONEL L. R. STANDIFER

I might add to the information that has been given to you that we are recording the symposium, and also that I shall be chairman of the fifth session. In that session, we expect to take up the four areas involved, one at a time. Each session chairman will first give a summary of his session and his viewpoints, based on what he heard, as to the requirements in this field, both technical facilities and magnitude of effort. Then, we will open the session to the panel and the audience for comments, especially as to how this particular session ties in with the subject matter of the other four sessions. These will be recorded and from this we hope to come up with a summary and guidance for the entire field, at least as far as the Air Force is concerned.

It gives me great pleasure, now, to open the first session by introducing your chairman of the session, Dr. Daniel C. Dr ker. He received his B.S. degree at Columbia in 1937, and his Ph.D. degree in Engineering in 1940. From 1940 until 1959, he moved progressively through the Mechanics Department to become Chairman of the Engineering Division and since 1960 has served as Chairman of the Physical Science Council at Brown University. He had a short association with the Air Force in 1945, which most all of us did, I guess, but we were fortunate in that he was assigned to the Flight Dynamics Branch of the Aircraft Laboratory which you people may remember. The societies to which he belongs would take up about a page of very fine print so I will not try to cover all of them. I would like to mention the top engineering research society, Sigma Xi. In addition to his university duties, Dr. Drucker has quite a technical literary career. At the present time he is technical editor of the Journal of Applied Mechanics for the American Society of Mechanical Engineers (ASME). He is a member of Committee Five of the Materials Advisory Board, the National Research Council, a Guggenheim Fellow from 1960 to 1961, and Chairman of the International Congress of Experimental Mechanics for 1961.

I always feel that it is superfluous to introduce a man of Dr. Drucker's stature. However, I did want to point out some of these areas which I feel make him eminently qualified to chair this first session on Mechanical Properties of Solids. Without further ado, Dr. Drucker.

DR. DANIEL C. DRUCKER

Chairman, Session I

I shall just give a very few comments on mechanical properties of solids which mean, of course, quite different things to different people, and you will hear about these different things at this very first session. We will go from physics of solids through I guess what is now called metaphysics, over to continuum mechanics with solids, and also actually start with fluid mechanics and thermodynamics as an approach to solids under very high impact conditions. So, we will have almost the complete range, then, from applied mathematics to physics.

The reason I am here is not because I am able to contribute; I think it's rather because I have so much to learn and there's no better way of learning than sitting up in the front and listening to the speakers that follow.

Our first speaker, Dr. Harold L. Brode, received his Ph.D. from Cornell University, and has been at RAND for 11 years. I think all of you know about the excellent work he has done in connection with very high energy explosions and other activities and he will speak to us on the one extreme which is the dynamic properties of matter in high stress from the consideration of thermodynamics and fluid mechanics. Dr. Brode.

ASD-TDR-63-140

DYNAMIC PROPERTIES OF MATTER UNDER HIGH STRESS - THERMODYNAMIC DESCRIPTIONS

by

Harold L. Brode, Ph.D. and A. C. Smith

The Rand Corporation

PREFACE

This report contains a brief description of the development and use of equations of state for solids. This research is part of a continuing investigation of ground shock cratering, and other phenomena of importance in studies of protective construction and weapons effects. This Memorandum was the basis of two briefings given by the authors. One was given by Smith at a "Conference on Equation-of-State" 10,11 September 1962 at the Air Force Special Weapons Center, Albuquerque, New Mexico. The other was given by Brode at an Air Force sponsored symposium on "Structural Dynamics Under High Impulse Loading" 17,18 September 1962 in Dayton, Ohio.

SUMMARY

Although solids behave elastically at very low stress levels, and exhibit complex visco-elastic and plastic behavior at higher stress levels, when stresses in the megabar range are applied, it is generally more meaningful to ignore stress tensor descriptions and turn to thermodynamic and fluid dynamic descriptions. At high enough stress levels the response of any material must be viewed as characteristic of a compressible fluid.

Some empirical information is available in the regions of stress of a few kilobars to a few megabars from high explosive shock experiments. Static compression data are useful guides at lower stresses, while at the highest levels one finds the properties are reasonably well determined by atomic models such as a Fermi-Thomas-Dirac temperature-dependent model. Theoretical calculations, numerical in nature, have provided the thermodynamic properties of all atomic elements. The properties of chemical mixtures as occur in natural materials can be constructed from suitable combinations of these elementary results. Successful use has been made of such results for calculations of dynamic response under high impulse loadings in a variety of applications. One particular example (for basalt) is illustrated.

Although all properties of matter are in theory derivable from their atomic origins, under more normal stress conditions the complexities of molecular interactions and of crystaline structure and solid state properties makes such an elementary derivation impractical, and at present impossible without a great deal more theoretical understanding. However, the properties of solids under high impulse become literally elementary when the stress level is high enough.

The shocks resulting from highly impulsive loads produce high temperatures as well as high stresses and compressions, and as the internal energy characterized by these high temperatures rises above the binding energies of atoms in molecules (and well above the heats of fusion of the solids) the properties of the heated matter can be described quite reliably in terms of the electronic properties of the individual atoms. Such a description, although a quantum mechanical one, can be conceptually quite simple in that it can ignore the many degrees of freedom and constraint appropriate to a colder solid state.

Of course, if the temperature is raised far enough the matter becomes a dense plasma of completely ionized or stripped nuclei and free electrons, and its thermodynamic properties become those of a nearly ideal gas of particles with no internal degrees of freedom (i.e., "mono-atomic"). Such a simple state is realizable when the temperatures are in excess of thousands of electron volts (tens of millions of degrees). Below such exhalted temperatures and down to levels much nearer an electron volt (tens of thousands of degrees) the properties of the atomic electrons dominate the thermodynamic behavior of matter. Thus a description which can account for the atomic behavior can be used to derive all the appropriate thermodynamic variables whenever the temperatures lie above a level where molecular binding energies cease to be important. This thermodynamic model has been extensively employed in dealing with high temperatures and high impulse loads; the familiar Thomas-Fermi equation which treats the electrons semi-classically, requiring them to obey Fermi-Dirac statistics provides the basis for such a description, and for this purpose, extensive numerical solutions have been made available.(1) These Fermi-Thomas equations, following fairly standard notations may be

expressed as follows:

$$\nabla^2 V(r) = 4\pi e\rho(r) ,$$

where

$$\rho(r) = \frac{8\pi}{h^3} \int_0^\infty p^2 dp \frac{1}{\exp\left[\frac{-\alpha + p^2/2m - eV(r)}{kT}\right] + 1} ,$$

for $kT \neq 0$,

$$\rho(r) = \frac{8\pi}{3h^3} \left[2m\,(\alpha + eV(r)\right]^{3/2}$$

for $kT = 0$ without exchange, and

$$\rho(r) = \frac{8\pi}{3h^3}\,(2m)^{3/2} \left[(eV + \alpha + \frac{2me^4}{h^2})^{1/2} + \frac{(2m)^{1/2} e^2}{h}\right]^3$$

for $kT = 0$ with exchange. These equations must obey the boundary conditions

$$\nabla V(r) = 0 \text{ at } r = r_0 ,$$

and

$$\lim_{r \to 0} rV(r) = Ze .$$

The solution relates the charge and electrostatic potential distributions within the atom. By using the virial theorem and thermodynamic definitions with these solutions, one may arrive at a prescription of pressure, internal energy, entropy, etc. as functions of the temperature (electronic) and of the specific volume (atomic).

In order to describe the equation of state of materials other than pure elements it is necessary to mix in appropriate mole

fractions the solutions for the various elements present in a compound or mixture, in an alloy or mineral. Ideally, this would be accomplished by solving the equations for the mixture, but aside from nuclear corrections this may be approximated by adding atomic volumes and specific energies of the individual elements at states of equal temperature (electronic) and of equal chemical potential which corresponds to states of equal pressures at the surface of the atoms.

From this mixture calculation one gets both the zero and nonzero temperature compression curves (pressure vs density) and can derive a Hugoniot curve (pressure vs density for a shock wave).

Since useful Hugoniot data exists from high explosive experiments in the range of tens of kilobars to a megabar, a range where the atomic (Fermi-Thomas) solutions become inaccurate, one can use the empirical (HE) data to correct the theoretical solutions and to extend them into lower stress regions. Although the usual scatter in data precludes any precise prescription for accomplishing such a correction, the following example indicates the essential steps.

A mixture calculation for basalt has been made by this method and a zero temperature curve was drawn (T-F, T = 0 in Fig. 1). Data from high explosive determinations[2] (Lawrence Radiation Laboratory, Livermore) representing basalt compressions at shock pressures between 100 and 800 kilobars were plotted along with two static compression (bulk modulus) data from Birch.[3] For guidance in arriving at a first guess to an extension and correction of the zero temperature curve, the zero-temperature and Hugoniot curves for aluminum were noted, as was the approximate sound speed (p-wave seismic velocity) in basalt. The latter should govern the slope of the compression curve at low stress levels.

This zero-temperature isotherm is extropolated into the tensile region with the required boundry conditions (1) that the slope is continuous, (2) that the pressure is zero at infinite volume, (3) that the maximum negative pressure be equal to the ideal tensile strength and (4) that the work done in taking the material to infinite dilution equals the heat of sublimation.

This first approximation to the zero-temperature compression curve and its accompanying energy curve, i.e.,

$$E_0(V) = - \int_{V_0}^{V} (PdV)_{T=0} + E_0^0$$

may be used to replace the Thomas-Fermi zero-temperature isotherm for pressures below ten megabars.

It is a reasonable presumption which can be partially supported that the difference between the Thomas-Fermi zero-temperature isotherm and the preceeding approximation can be applied as a correction to the pressure and energy equations of state for non-zero temperatures. At high temperatures it is nearly correct, being complicated by other non-ideal features both thermodynamic (phase changes) and solid state (crystaline structure changes) which affect the energy as a function of temperature.

$$P(T,V) = P_{TF}(T,V) - P_{TF}(V)_{T=0} + P_0(V) + P_{Nuclear}(T,V)$$

$$E(T,V) = E_{TF}(T,V) - E_{TF}(V)_{T=0} + E_0(V) + E_{Nuclear}(T,V).$$

One of the simplest treatments of the nuclear contributions is to treat the nuclei as a perfect gas in gaseous phases, and as a perfect solid in solid phase regions.

With this corrected energy and pressure one can calculate a Hugoniot compression curve from the shock condition

$$E_s - E_0 = \frac{P_s + P_0}{2} (V_0 - V_s)$$

and compare it with the (HE) data already plotted. This comparison may suggest further arbitrary changes in the zero temperature curve in order to arrive at closer agreement with the shock data.

It is possible to include the proper energetics of phase changes by computing for two states, one determined with solid compressibilities and sound speeds and the other using liquid compressions

and sound speeds. The appropriate state is then determined (as a function of temperature and density) as that state having the lowest free energy; the transitions from one equation of state to the other require appropriate internal energy changes.

The second figure indicates the caloric equation of state thus derived for basalt. The factor $\frac{PV}{E}$ displayed for various isotherms as a function of compression is in essence the factor γ-1 recalled from ideal gas notation.

The limitations to the usefulness of such equations of state are associated with the regions of validity of hydrodynamic assumptions. The most outstanding improvement would be reasonable extension into the regions of plasticity and elasticity, which presumes, then, satisfactory treatment of such features as cracking, crushing, void destruction, and viscous flow as well as providing for a logical broadening of the equation of state to encompass tensor stresses and an extension of the descriptive properties to include a knowledge of its history in order to properly identify the current state of the material (when hysteresis effects exist).

Although the direction to go to extend thermodynamic descriptions into solid state regions may be clear, it is also clear from some previous solutions to interesting high impulse problems that a great deal of useful information can be provided by a straightforward hydrodynamic description of the early or intense phases.

A simple addition to the hydrodynamic treatment can be the inclusion of tensile failure which might be called destructive cavitation, after which the material is a gravel and will not support tension. That such extensions and broadening of thermodynamic descriptions can be meaningful has already been partially demonstrated in numerical calculations that have proceeded from the hydrodynamics of intense explosions in solids into plastic and elastic wave propagation computations.[4] The calculations generally employ simple changeover criteria such as a critical stress level, which, if not exceeded, dictates elastic response. But to go directly from hydrodynamics to elasticity by such an arbitrary scheme is to ignore the influences of many physically important responses (crushing, cracking, plastic

flow), and further effort is needed to include adequate modeling of these features.

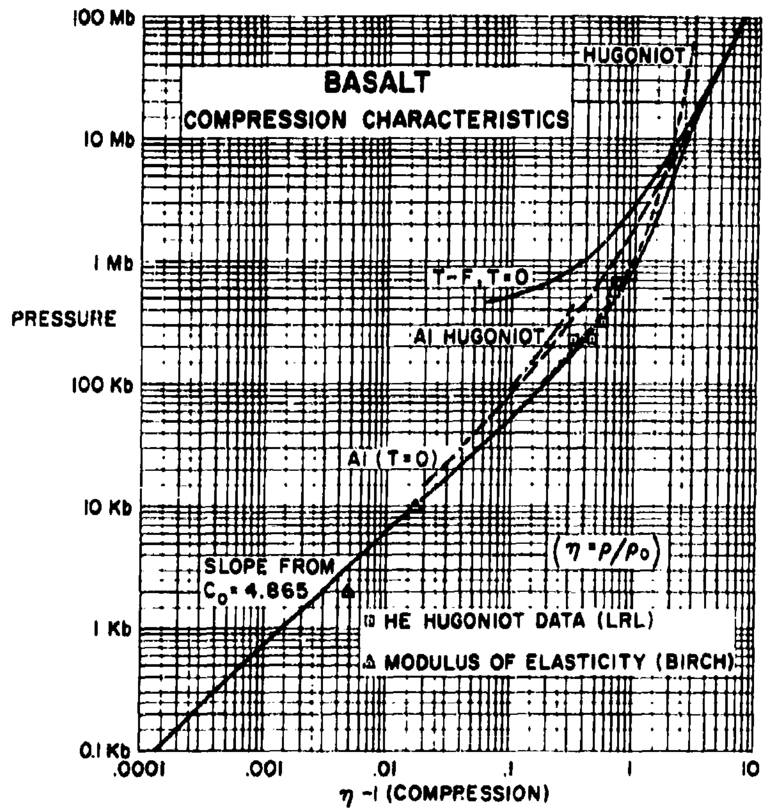

FIGURE 1. BASALT COMPRESSION CHARACTERISTICS

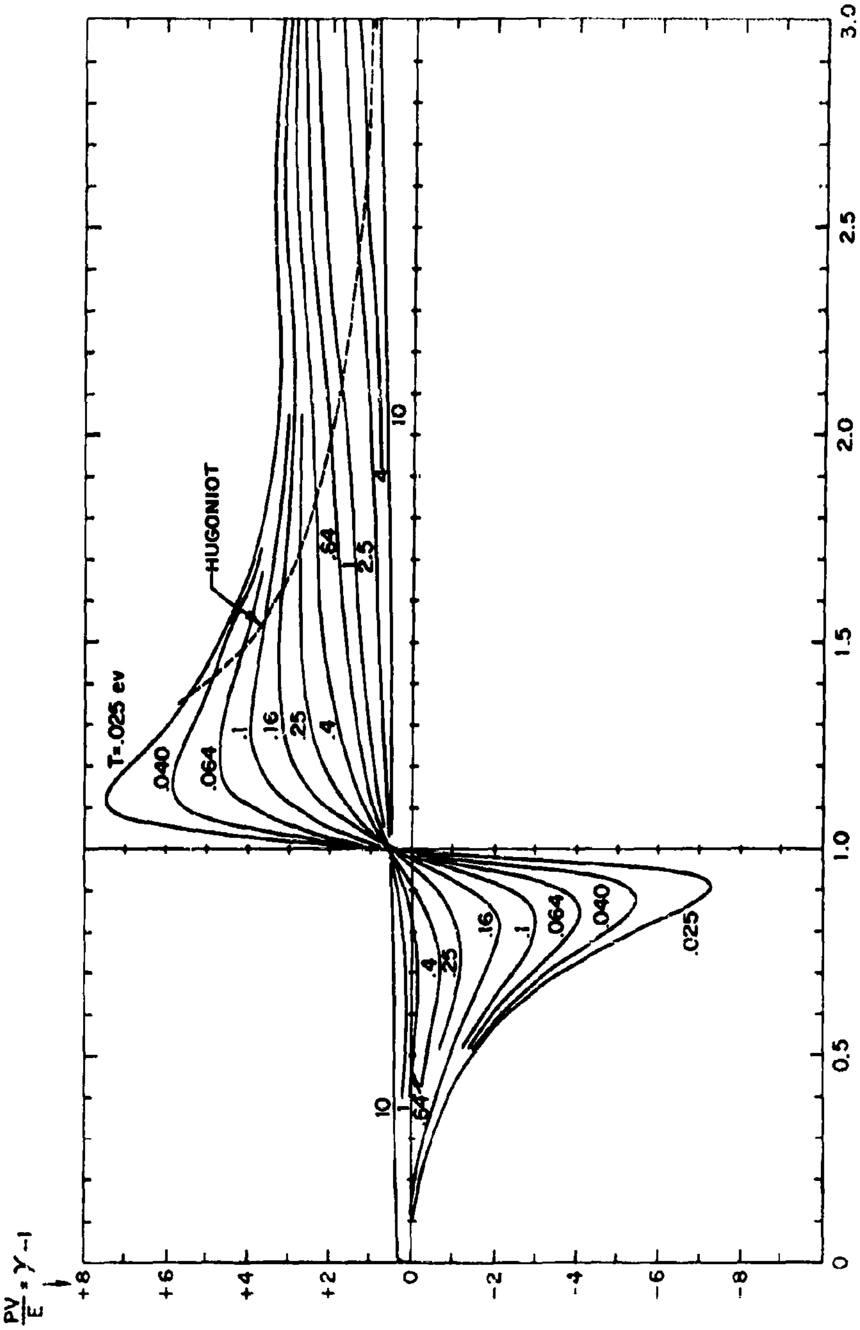

FIGURE 2. EQUATION OF STATE FOR BASALT (ISOTHERMS)

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2. Lombard, David B., The Hugoniot Equation of State of Rocks, University of California, Lawrence Radiation Laboratory, UCRL-6311, 1961. (In addition, some more recent LRL data was included.)

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DISCUSSION

DR. DRUCKER

Of course, this symposium is devoted to the great difficulties experienced by metals and other materials under very adverse conditions and we should probably say very adverse conditions on the speakers as well. Does anyone have a question or a comment they would like to make? I think it would help if you would identify yourself for the benefit of the person taking notes.

DR. HERRMANN, MIT

I think in your equation you took the Grüneisen ratio to be two. Does this apply to basalt and can it be different for other materials and if so, would you comment on how you get it?

DR. BRODE

I'm not sure I understand the question.

DR. HERRMANN

You have an equation in which you related the energy densities of the pressure offset from the zero degree isotherm. That was written as (repeated an equation). The 2 is the Grüneisen ratio?

DR. BRODE

No, that is the proper expression for a hydrodynamic jump condition which would apply in any material for any equation of state for any ratios, Grüneisen, and so on. This is a presumption of the fluid dynamics, of course, that for continuity, the equations for conservation of mass and energy are expressed by addition to an equation of state which is simply a statement of relating energy to pressure and density, so that is not a special assumption.

ANDERSON, AEROJET

What fraction of your compressible energy do you consider goes into thermal energy in the range of several hundred kilobars up to about a megabar?

DR. BRODE

The question was: What fraction of the energy in the range of several hundred kilobars goes into thermal energy? It's essentially all thermal energy, of course; that is, compressional energy which you can get useful work back out of; but it depends entirely on where in this system of adiabats or isotherms you are, and how you arrived there. If you arrived on the shock, a fraction of this is compression and a fraction is heat. If you go on an

adiabat, of course, you can get it all back around that adiabat. In other words, in adiabatic compression presumably you can arrive at exactly the same place you started. If it's a shock, and in most of the practical examples you arrive at, it's a shock heating process.

ANDERSON

I'm talking about the reversible part, and if all the compressional energy goes to thermal you can equate your Hugoniot energy simply to a difference in thermal energy, for instance, $C_p \Delta T$. However, if part is tied up intermolecularly then you must extract that from it. This has been an important controversial question for quite a long time.

DR. BRODE

I'm not sure I interpret the question. For instance, in this whole compression curve which I drew, from which a Hugoniot departs and actually arrives at some kind of maximum possible compression, that if one were to draw the subsequent isotherms in here or adiabats which depart from this Hugoniot, then you can quickly see that having shot something to an appreciable stress level in excess of hundreds of kilobars, you have departed considerably from the cold temperature linear behavior and you will arrive, after you have re-expanded to normal volume, at a much higher temperature. The adiabats will leave you on a higher temperature level. When you're all through and get back to near normal volume and zero pressure you will still have this thermal heat which is essentially represented by how far the Hugoniot has taken you from your zero isotherm. This is a continuous function of the properties of the material, however, and as you go to higher and higher stress levels, more and more of the energy winds up in a kind of waste heat in the thermal energy which cannot be retrieved by re-expansion. I don't know that you can draw a pure line of what this fraction is except as a function of the particular material and the particular stress level. I don't think I answered that to your satisfaction.

DR. DRUCKER

Any other questions or comments?

FROM THE FLOOR

I was just curious as to how the Hugoniot data was taken. What experimental technique was used at Livermore?

DR. DRUCKER

Did everybody heat that question? "How was the data taken and what experimental technique was used at Livermore?"

DR. BRODE

Their experiments generally use a slab technique, plane wave kind of techniques where they measure both the shock velocity and the particle velocity, or free field. Actually, they measure the velocity at the end of a plate which is twice the particle velocity sort of thing. From this, of course, they get directly the pressure from the Hugoniot relations which state that the pressure is essentially the density times the product of the particle velocity and the shock velocity. Having the pressure and being able to compute from this ratio of the particle velocity to shock velocity, the compression, then they have a point, essentially on this compression curve, that is, p equals shock velocity. The ratio of particle velocity to shock velocity is essentially one over . . . (inaudible) . . . minus one over the compression for the material, so you have a point from any one experiment then. Having measured both the particle velocity and the shock velocity, which is a matter of measuring in the shock velocity case (the transit time usually across a known material) you have a measurement of the material compression.

W. O. DAVIS, HUYCK CORPORATION

Has there been any effort made to separate the magnitude of the pressure from the rate of onset of pressure? In other words, normally, in a shock wave, the velocity will determine both, so you will have some sort of unique relationship between the two. Has anybody specifically made an effort to separate those two variables?

DR. BRODE

The question was: "Has there been any attempt to separate the rate of rise and the peak pressure as separate measurables in such experiments?" I think the answer is yes. I am not an expert. There is a finite rise, of course, in any shock, and particularly in shocks at lower levels in solids you get into many features such as essentially multiple shocks or step rises. You get even into the elastic recursor problem which is, I suspect, a subject for other discussion.

KENNON, ARMOUR RESEARCH FOUNDATION

Did your model over here match the theory with these Hugoniot data points? Is that what your figure showed?

DR. BRODE

The question is, "Does our model match the theory with the data points?"

KENNON

That is number 1.

DR. BRODE

We corrected the theory in the low temperature range, or the low pressure range, actually, by this empirical process of fitting it to the data. We required it to be compatible with the data in this range, in addition to the requirements that I suggested for the negative pressure phase (a tension phase). At the tension point then we also crank in the energies of phase change.

KENNON

Would there be a possibility of synthesizing the Hugoniots--that is, your data was essentially for the Hugoniot of the basalt that you pictured there, and your theoretical model essentially is for a mixture, but not a mixture of crystals; that is, you had a solution, or a mixture of elements?

DR. BRODE

The question has to do with the compatibility of the theoretical model and the actual data. The data is taken for basalt in a 40-mile canyon in the Nevada test site. The theory was compounded for an atomic mixture. Its atomic constituents are appropriate to that of basalt. The question is whether this is appropriate considering crystalline structure, and so forth. The answer is obviously yes because the Fermi-Thomas part of the solution, where we rely on that as a reliable answer to the thermodynamic behavior, has no crystals. You are at temperatures of tens of thousands to millions of degrees and you are now dealing with the properties of individual atoms. The properties of the material now are the properties of the atoms, correctly specified. Now, when you get in this range where this data is taken, you are now working on crystals and it does make a difference. Therefore, we must pay attention to this data and ignore the simple atomic model. That's why we made this correction to the theory to extrapolate our atomic theory into lower temperature regions where we can usefully extrapolate the calculations; but it is appropriate in the high temperature region to use the Fermi-Thomas. It is a perfectly legitimate model which has been demonstrated to be extremely useful; after all, it is the basis of our weapons design work that we have models and the materials in just this way, and characterize their compressibilities by just such techniques. It worked very well, but only because this is good in high stress models. It is not good in the lower stress models. There are other reasons of concern here other than on the Hugoniot where crystal behavior is different, but that's another discussion. Would you want me to go on to that?

DR. DRUCKER

Let's move on with the discussion. I'm sure this would continue for a long, long time. And thanks once again for an extremely interesting talk and very interesting answers to the discussion which followed.

The next speaker on the program is Professor J. L. Ericksen of Johns Hopkins University, who received his Ph.D. degree from Indiana University. His interest is on the highly mathematical side of continuum mechanics. He will speak to us on what he calls Oriented Solids. Dr. Ericksen.

ORIENTED SOLIDS

by

J. L. Ericksen, Ph.D.

The Johns Hopkins University

ORIENTED SOLIDS

J. L. Ericksen

Mechanics Department, The Johns Hopkins University

ABSTRACT

Roughly, a continuum theory of oriented materials is one which ascribes a structure to a material point. This concept is useful for combining ideas concerning microscopic structure with thoughts concerning the macroscopic behavior of materials. Our purpose is to describe some theories of this type which show promise as theories to describe nonlinear mechanical behavior of solids.

ORIENTED SOLIDS

INTRODUCTION

Abstractly, an oriented continuum is a continuum consisting of material points, to which are attached vectors, hereafter called directors, or more complex mathematical entities involving preferred directions. When subject to loads, such materials may move and their directors may change quite independently. Use of this concept eases incorporation of ideas concerning geometric or microscopic structure in continuum theories. With respect to geometric structure, the common situations concern one or two dimensional theories of rods or shells. Thus a rod is idealized as a curve endowed with directors commonly interpreted as the principal geometric axes of inertia of the rod's cross sections. With these, one can describe resistance to twisting motions leaving the curve unaltered. Further discussion of and reference to literature on such theories is given by Ericksen and Truesdell [1]. Here we concentrate on mechanical theories wherein directors are more naturally identified with microscopic structure. Such mechanical theories seem to be less well known than are corresponding electromagnetic theories of rigid or deformable dielectrics, wherein a polarization vector is introduced to represent structure roughly like that of an electric dipole. Recently, interest in mechanical theories of oriented materials has increased because of promise they show in describing complex mechanical behavior of solids and non-Newtonian fluids.

It is impracticable to give here a comprehensive review of work on theories of oriented materials. Rather, we describe researches giving some rough idea what such theories entail. Cartesian tensor notation is used throughout.

SECTION I: COSSERAT MATERIALS

The work of the Cosserats [2], published in 1909, represents the first attempt to formulate a reasonably general nonlinear, three dimensional theory of oriented solids. It was presented as a mathematically natural generalization of theories of rods and shells rather than as a theory designed to describe a specific real material. We present a modification of theory which seems to be closer to and, in some respects, more advanced than other theories currently being developed.

Motion of a continuum can be described by giving the present coordinates x_i of a particle as functions of its initial coordinates X_α and time t

$$x_i = x_i(X_\alpha, t). \tag{1}$$

In addition, we introduce N vectors, the directors, denoted by

$$d_{ai} = d_{ai}(X_\alpha, t), \qquad a = 1, \dots, N. \tag{2}$$

The theory is based on a variational principle involving a "Euclidean action" W of the form

$$W = W(X_\alpha ; x_{i,\beta} ; \dot{x}_j ; d_{ak} ; \dot{d}_{bq} ; d_{cm,\nu}), \tag{3}$$

where commas denote partial differentiation and the dot denotes the material derivative. It is understood that the material is in some standard state initially. The principle states that

$$\delta \int_{t_1}^{t_2} \int_{V_0} \rho_0 W \, dV \, dt = \int_{t_1}^{t_2} \oint (T_i \, \delta x_i + T_{ai} \, \delta d_{ai}) \, dS \, dt + \int_{t_1}^{t_2} \int_{V_0} \rho_0 (f_i \, \delta x_i + \varphi_{ai} \, \delta d_{ai}) \, dV \, dt, \tag{4}$$

wherein ρ_0, the initial density, V_0, the initial volume and $t_2 - t_1$, are not to be varied. On the right hand side, T_i, T_{ai}, f_i and φ_{ai} denote generalized applied forces. The Cosserats assume $N = 3$, the three directors being constrained to be mutually orthogonal unit vectors so that

$$d_{ai} \, d_{bi} = \delta_{ab}. \tag{5}$$

Variations permitted in (4) must then be consistent with (5). These and other such algebraic constraints can be handled by the common devices of expressing all variations in terms of some independent set or by suitably introducing Lagrange multipliers. We restrict our attention to cases where there are no such constraints.

It is now a matter of routine to grind out differential equations and boundary conditions in various alternative forms. The Cosserats do this in some detail for cases which they considered, so we shall be brief. One formulation is illuminating with respect to the rather unusual forms of the laws of conservation which might reasonably be expected to hold for oriented media. For this purpose, we define an energy e by

$$e = -W + \frac{\partial W}{\partial \dot{x}_i}\dot{x}_i + \frac{\partial W}{\partial \dot{d}_{ai}}\dot{d}_{ai} \, . \tag{6}$$

Physically, e is interpretable as total energy per unit mass under adiabatic conditions. We also recast the differential equations in equivalent Eulerian integral forms. We obtain the following types of equations:

$$\frac{d}{dt}\int_v \rho p_i \, dv = \oint t_{ij}\, da_j + \int_v \rho f_i \, dv, \tag{7}$$

$$\frac{d}{dt}\int_v \rho \pi_{ai} \, dv = \oint T_{aij}\, da_j + \int_v \rho(\varphi_{ai} + \psi_{ai})\, dv, \tag{8}$$

$$\frac{d}{dt}\int_v \rho e \, dv = \oint (t_{ij}\dot{x}_i + T_{aij}\dot{d}_{ai})\, da_j + \int_v \rho(f_i\dot{x}_i + \varphi_{ai}\dot{d}_{ai})\, dv, \tag{9}$$

$$\frac{d}{dt}\int_v \rho l_{ij} \, dv = \oint m_{ijk}\, da_k + \int_v \rho l_{ij}\, dv, \tag{10}$$

where v is any material volume and ρ is the present density, determined so that mass is conserved

$$\frac{d}{dt}\int_v \rho \, dv = 0. \tag{11}$$

In (7), we have

$$p_i = \frac{\partial W}{\partial \dot{x}_i} \quad , \quad t_{ij} = -\rho \frac{\partial W}{\partial x_{i,\alpha}} x_{j,\alpha} \; . \tag{12}$$

It is relatively clear that (7) represents the law of conservation or balance of linear momentum, t_{ij} being the stress tensor, f_i the body force. It reduced to the form commonly used in nonlinear elasticity when W is of the form

$$W = \tfrac{1}{2} \dot{x}_i \dot{x}_i + f(x_{i,\alpha}) , \tag{13}$$

f being the negative of the strain energy. In this case, the stress tensor is symmetric, as is commonly the case in mechanics. For the theories here considered, the stress is rarely symmetric, as can be seen from (12).

Equation (8) is rather similar to (7). In it

$$\pi_{ai} = \frac{\partial W}{\partial \dot{d}_{ai}} \; , \; \tau_{aij} = -\rho \frac{\partial W}{\partial d_{ai,\alpha}} x_{j,\alpha} \quad , \quad \psi_{ai} = \frac{\partial W}{\partial d_{ai}} \; , \tag{14}$$

so π_{ai} is interpretable as a momentum associated with motion of d_{ai}. What corresponds to the body force f_i is somewhat different, involving an intrinsic part ψ_{ai} which need not vanish when externally applied forces do.

Equation (9) is clearly recognizable as a statement of the law of conservation of energy. What is added to more conventional forms are

$$\tau_{aij} \dot{d}_{ai} \quad , \; \varphi_{ai} \dot{d}_{ai} \; ,$$

acknowledging the fact that work will be done in moving the directors. From a conventional point of view, (9) is deficient in that it does not allow for changes in energy associated with flow and generation of heat.

Equation (10) can be deduced from the other equations, together with the assumption that W is not affected by

static rigid rotations. That is we assume that W is unaltered when we make the substitution

$$x_i \to R_{ij} x_j \quad , \; d_{ai} \to R_{ij} d_{aj} \; ,$$

where R_{ij} represents a rigid rotation. From this, (12) and (14), one can show that the tensor

$$P_{ij} \equiv \rho \dot{x}_i p_j + \rho \dot{d}_{ai} \pi_{aj} - t_{ji} - d_{ai,k} \tau_{ajk} + d_{ai} \psi_{aj} \tag{15}$$

satisfies the identity

$$P_{ij} \equiv P_{ji} \tag{16}$$

which represents a generalization of the more common statement that the stress is symmetric. Using (7), (8) and (16), one can deduce that (10) holds with

$$h_{ij} = x_i p_j - x_j p_i + d_{ai} \pi_{aj} - d_{aj} \pi_{ai} \; , \tag{17}$$

$$m_{ijk} = x_i t_{jk} - x_j t_{ik} + d_{ai} \tau_{ajk} - d_{aj} \tau_{aik} \; , \tag{18}$$

$$l_{ij} = x_i f_j - x_j f_i + d_{ai} \varphi_{aj} - d_{aj} \varphi_{ai} \; . \tag{19}$$

From this, it is clear that (10) represents a statement of the law of conservation of moment of momentum, h_{ij} being the moment of momentum, m_{ijk} representing surface couples, and l_{ij} representing body couples.

Very little has been done to develop these theories mathematically. Most of what is known concerns the applications of the static version with but one director to one class of materials, the liquid crystals. Here the director is interpreted as describing the orientation of liquid crystal molecules or sets

of these. Generally, these are relatively large, rigid rodlike molecules. Here the surface forces $\tau_{\alpha i}$ seem to be poorly understood. To some degree of satisfaction, one can calculate and produce non zero values of $\varphi_{\alpha i}$ in cases where it is produced by simple electromagnetic fields. Changing these has an observable effect on orientation. Some rather satisfactory analyses of observations have been made. Since these materials are quite unlike metals or other common solids, it does not seem worthwhile to describe these researches in detail. Those interested might consult Ericksen [3] or Frank [4] for further discussion and references.

It would be rash to expect such theories to apply to dissipative systems. For these, it seems reasonable to modify (9) to properly account for heat effects and to adopt (7)-(11) and (17)-(19) as a general framework. The problem is then to devise more appropriate constitutive equations to replace (12) and (14).

SECTION II: DISLOCATION DISTRIBUTIONS

It is generally believed that, in crystalline solids, macroscopic phenomena commonly considered to fall under the heading of plasticity somehow have their origins in the movement of dislocations and other imperfections. The theory of continuous distributions of dislocations is being developed to facilitate introduction of ideas concerning such microstructure into macroscopic theories of solids. Pertinent kinematics and geometry have been studied extensively and are now rather well understood. Comprehensive expositions are available, one of the more recent being that of Bilby [5]. The question of how forces should be treated is more unsettled. Here one needs in part the general framework represented by our equations (7)-(10) and (17)-(19) or some variant thereof. As is discussed by Kroner [6], there is reason to expect the stress tensor to sometimes be asymmetric, so a more conventional framework may not apply. Also needed are properly formulated constitutive equations somewhat like, thought doubtless different from, those given by our equations (12) and (14).

To give some indication of what such theories involve, we outline a complete, though admittedly oversimplified version of the theory proposed by Santiago [7]. He intends to describe ideal materials which do not work harden. For this, we introduce three directors $d_{\alpha i}$. Loosely speaking, these may be identified

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with suitable average values of lattice vectors in a crystal. When the crystal is deformed elastically, it is commonly assumed that these are carried along with gross motion in the sense that

$$d_{ai} = x_{i,\alpha} D_{a\alpha} , \tag{20}$$

where the $D_{a\alpha}$ are initial values of the lattice vectors. Equivalent to (20) is the statement that, at all times,

$$\dot{d}_{ai} = \dot{x}_{i,k} d_{ak} . \tag{21}$$

When plastic deformation occurs, it is assumed that the directors remain smooth functions of position and time, but are not carried along with the gross motion. Roughly, the macroscopic deformation takes place largely by slip or other imperfection motion, so that the crystal lattice is less distorted than it would be under an elastic deformation of the same size. This suggests that we write

$$\dot{d}_{ai} - \dot{x}_{i,k} d_{ak} = - W_{ij} d_{aj} , \tag{22}$$

where W_{ij} is a measure of rate of plastic deformation, being zero when the material is deforming elastically. For W_{ij} , Santiago assumes a constitutive equation of the form

$$W_{ij} = W_{ij} (d_{ak} ; \theta ; \theta_{,m}) , \tag{23}$$

where θ denotes the temperature. Equations (22) and (23) represent a replacement for or perhaps some approximation to our equation (8). Santiago uses the conventional form of the energy equation

$$\rho \dot{\epsilon} = t_{ij} \dot{x}_{i,j} - q_{k,k} ,$$

where ϵ represents internal energy, q_k the heat flux vector,

together with an equation of state of the form

$$\epsilon = \epsilon(d_{ak}, \eta) \tag{24}$$

where η is entropy density. Also he introduces a constitutive equation of the form

$$q_k = q_k(d_{am}; \theta; \theta_{,k}), \tag{25}$$

generalizing Fourier's law. When (20) holds, these equations reduce to forms commonly used in thermoelasticity theory. He also assumes that

$$\theta = \frac{\partial \epsilon}{\partial \eta} \tag{26}$$

and

$$t_{ik} = \rho \frac{\partial \epsilon}{\partial d_{ai}} d_{ak}. \tag{27}$$

Equation (26) is clearly borrowed from the thermostatics of reversible phenomena. Equation (27) is introduced on the intuitive grounds that the stress is primarily due to lattice distortion in the absence of work hardening. This, or something very similar, seems to be becoming standard among workers in the field. These equations, plus (7) with $p_i = \dot{x}_i$ complete the theory, at least in outline. From them, one can deduce an equation for the rate of production of entropy,

$$\rho \theta \dot{\eta} = t_{ij} w_{ij} - q_{k,k}. \tag{28}$$

The constitutive equations are to be specified so that the Clausius inequality always holds in the form

$$\rho \dot{\eta} + (q_k/\theta)_{,k} \geq 0. \tag{29}$$

Sufficient for this is that

$$\theta > 0 \,, \quad t_{ij} w_{ij} \geq 0 \,, \quad q_k \theta_{,k} \leq 0 . \tag{30}$$

Considering that the strict inequalities in (30) sometimes hold, the theory thus accommodates the irreversibility inherent in plastic deformation and heat conduction, whereas the Cosserat theory described in SECT. I appears to be suited for reversible phenomena only. In another respect, the Cosserat theory is more realistic in that it will generally predict many states of stress in which the material can be at rest. Examination of (22), (23) and (27) indicates that such states of stress are exceedingly rare for Santiagos materials. Basically, it is for this reason that the latter cannot be expected to cope with work hardening. It is not yet well enough developed for me to assess what it will do. What one does to remedy this will depend somewhat on what microscopic theory of work hardening one accepts. For example, Taylor's [8] theory, involving regular lattices of defects moving somewhat independently of the atomic lattice, might suggest introducing additional directors to describe the defect lattice. Apparently, this possibility remains to be considered.

We content ourselves with this rather inadequate sketch of this area of research.

SECTION III: A PROTOTYPE

Some time ago, I noticed that some of the theories which I [9] had proposed to describe oriented or anisotropic fluids predict behavior rather like that which can be observed in some real solids, for example, carefully annealed aluminum polycrystals. The mathematical structure of these theories is somewhat similar to that of the theories of continuous distributions of dislocations. We sketch these in the hope that it may provide food for thought as a prototype of yet to be developed theories of plasticity.

The theory involves a single director, which we label n_i, satisfying equations somewhat like (22) and (23). More precisely, there are equations of the form

$$\dot{n}_i - \dot{x}_{i,k} n_k = (\mu_1 + \mu_2 d_{km} n_k n_m) n_i + (\mu_3 - 1) d_{ij} n_j \,, \tag{31}$$

where the μ 's are functions of $n^2 = n_i n_i$ and

$$2 d_{ik} = \dot{x}_{i,k} + \dot{x}_{k,i} \tag{32}$$

is the rate of deformation tensor. We restrict our attention to incompressible materials, for which the stress tensor is given by equations of the form

$$\begin{aligned} t_{ij} = t_{ji} = -p\delta_{ij} + (\lambda_1 + \lambda_2 d_{km} n_k n_m) n_i n_j + \lambda_3 d_{ij} \\ + \lambda_4 (d_{ik} n_k n_j + d_{jk} n_k n_i), \end{aligned} \tag{33}$$

where p is an arbitrary pressure and the λ 's are functions of n^2 .

From these ideal materials, we wish to select those which can support shearing stresses of various magnitudes statically. This requires that, simultaneously,

$$\lambda_1 n_i n_j \neq 0 \ , \ \mu_1 n_i = 0$$

which we satisfy by taking

$$\mu_1 \equiv 0, \ \lambda_1 \not\equiv 0. \tag{34}$$

We consider that, initially, the material is isotropic, or nearly so, which we interpret to mean that

$$n_i \approx 0 \ \text{at} \ t = 0. \tag{35}$$

We wish to describe materials which work harden. Roughly, this means that there are unstressed states which are somehow distinguishable from each other. One possibility is that they correspond to different values of n_i . Exploring conditions that the material be statically unstressed, we see that this requires that

$$\lambda_1 = 0 \ \text{for some values of} \ n^2. \tag{36}$$

For simplicity, consider the special case where μ_2 and μ_3 are constants such that

$$\mu_3 > 1 \; , \; \mu_2 < 0 \tag{37}$$

and suppose, as is consistent with the above remarks, that $\lambda_1 n^2$ is positive for small values of n^2, that it attains a maximum, then drops to a negative minimum, then increases to a new positive maximum higher than the first. It may then drop to another minimum, approach a still higher maximum, etc.

One of the simplest situations to consider is that of shearing motions of the form

$$\dot{x}_1 = \dot{\gamma}(t)\, x_2 \; , \; \dot{x}_2 = \dot{x}_3 = 0, \; \dot{\gamma} \neq 0. \tag{38}$$

In this case, it is a simple matter to integrate (31). With any given set of initial conditions, one obtains a definite relation between n_i and the shear strain $\gamma(t)$. It turns out that essentially all solutions consistent with (35) coincide with or are closely approximated by solutions of the form

$$\left.\begin{aligned} n_1 &= k_1\, n \; , \; k_1 = \sqrt{(\mu_3+1)/2\mu_3} \; , \\ n_2 &= k_2\, n \; , \; k_2 = \sqrt{(\mu_3-1)/2\mu_3} \; , \\ n^2 &= -\frac{\mu_3}{\mu_2} \; \frac{C \exp\left(\sqrt{\mu_3^2-1}\, \gamma\right)}{1 + C \exp\left(\sqrt{\mu_3^2-1}\, \gamma\right)} \; , \end{aligned}\right\} \tag{39}$$

where C is a positive constant, which should be taken small for consistency with (35). The possibility that $C = 0$ exists, but is not representative, corresponding to an unstable situation. The corresponding shear stress $\tau = t_{12}$, from (33),

$$\tau = \tau_3 + \mu \dot{\gamma} \; , \tag{40}$$

where

$$\tau_s = \lambda_1 n_1 n_2 = k_1 k_2 \lambda_1 n^2, \tag{41}$$

$$2\mu = 2\lambda_2 n_1^2 n_2^2 + \lambda_3 + \lambda_4 n^2. \tag{42}$$

We assume that $\mu > 0$. Using (39), we can express the "static shear stress" τ_s as a function of the shear strain γ. From our assumptions concerning λ_1, the graph of this function will be like that indicated in Figure 1, wherein the dashed lines are disregarded.

Now consider how such a material would behave if we were to apply a fixed shearing stress smaller than the value $\bar{\tau}$ of τ_s corresponding to the first maximum in Figure 1. Initially, (40) requires that

$$\tau \approx \mu \dot{\gamma}$$

That is the material starts to move. As the strain increases with time, (39) indicates n^2 will increase. In turn, this means that τ_s will increase. As it does, (40) implies that $\mu\dot{\gamma}$ will decrease, ultimately approaching zero. If we are interested only in the long time behavior and only in stresses smaller than $\bar{\tau}$, we can equate τ and τ_s, using the early part of the stress-strain curve in Figure 1. Under these conditions, the material will behave as a somewhat nonlinear elastic material, the curves for loading and unloading coinciding. If we so load the material up to a load slightly less than $\bar{\tau}$, then increase it to $\bar{\tau}$, a new type of behavior results. It will not follow the static stress strain curve, but will follow the dashed line in Figure 1, the material coming to rest when the strain attains the value corresponding to the intersection of this line with the static stress strain curve. If the loads are then reduced slowly, the dashed line will not recur, rather the stress will move down the neighboring part of the stress strain curve. In short, permanent deformation occurs. If we increase the load slightly, we move up the neighboring part of the stress strain curve until we reach the second maximum, etc. Recent experimental data much like this, obtained in torsional experiments are reported by Dillon [10]. In terms of the mathematical model, the data would imply numerous maxima. In (39), we note that n^2 approaches a finite value as $\gamma \to \infty$. From the theory, this might be expected to occur at a finite stress. This situation has not

been carefully assessed. Superficially, it appears that the ideal material would flow indefinitely if this stress is exceeded.

At present, it is not clear how best to interpret the single director in microscopic terms. Crudely, it is natural to think in terms of measuring changes in structure and orientation of grains. It would be illuminating to know what microscopic changes occur during periods where permanent deformation develops in "static" experiments of the type just described.

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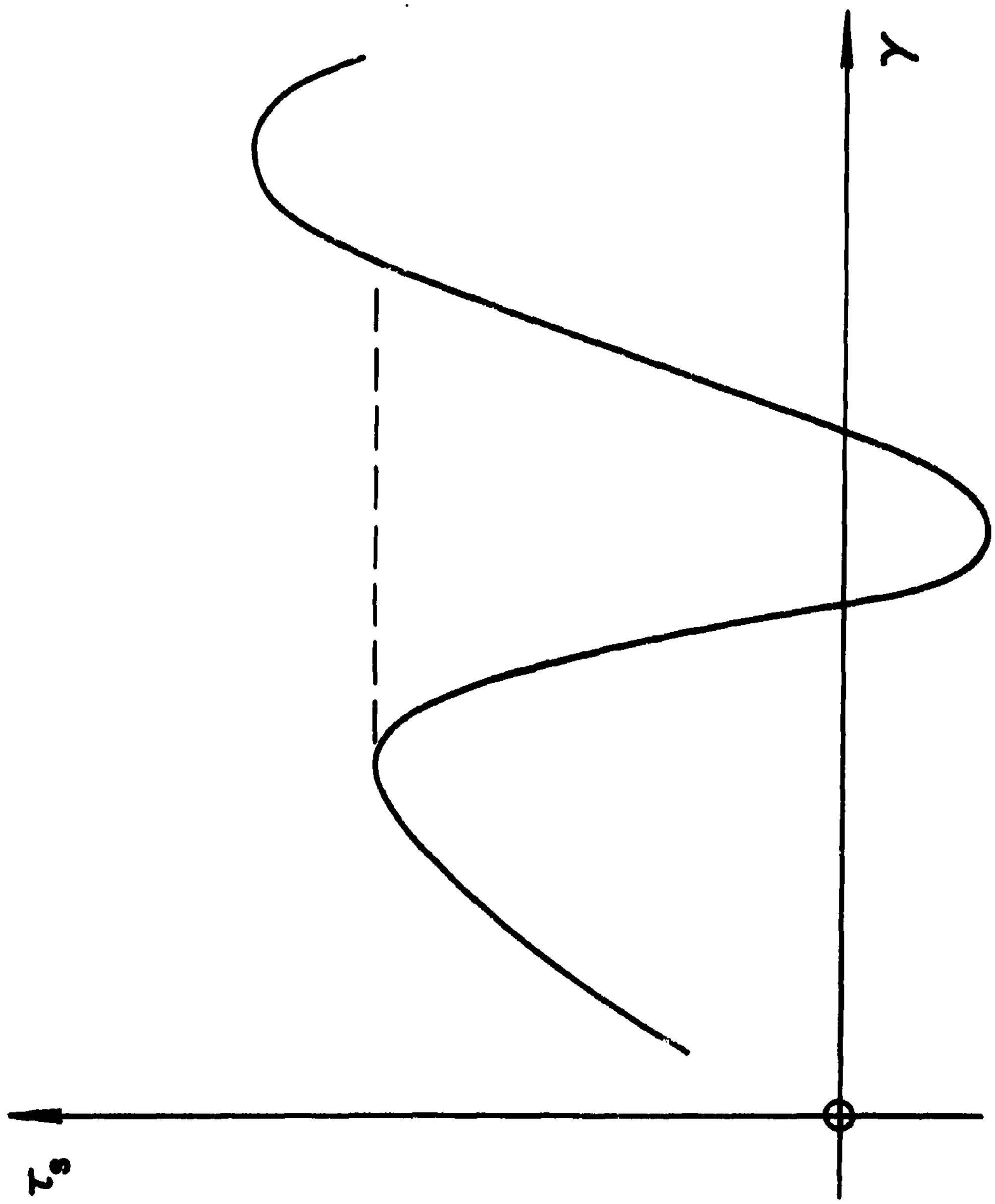

FIG. I STATIC STRESS-STRAIN CURVE

DISCUSSION

DR. DRUCKER

Dr. Ericksen's paper is open for discussion and comment. Do we have floor mikes here? Will you rise and state your name?

EUBANKS, ARMOUR RESEARCH FOUNDATION

With the addition of initial conditions and boundary conditions, you appear to have a couple of completely formulated theories. Have you investigated the question of uniqueness? I admit that these may be tied into loading tests, but it would seem to me that with such a completely formulated theory that one way of investigating this relationship to real life is to question the conditions which one must have before uniqueness of any solutions or of any deductions.

DR. ERICKSEN

The question oversimplified was whether I had investigated uniqueness and I would take it as stability conditions in connection with some of the theories which are rather completely formulated. The answer is "slightly, but not at all completely." Now, I oversimplified the statement of what we did in analyzing this. What was done to analyze this could be interpreted somewhat physically as follows. In terms of initial conditions, the situation we started with was a material which was almost isotropic. In terms of these directions, that means the directions were zero, or close to zero initially, and the analysis, then gives you a result which depends slightly on what you actually assume about these things, but they all converge into a region, all move along closely together here with the exception of the singular case where it is exactly isotropic to begin with. And that's an unstable possibility. The stability there is analyzed in a fairly simple-minded way, in a way that I think is a realistic and reasonable way in terms of mathematics for the situation. With respect to uniqueness, the uniqueness question really isn't at all well settled in anything like say---the elasticity theory, and for this sort of thing, well, probably for experimental reasons, I think this is a tough problem. The reason perhaps is not so much an ignorance of the uniqueness stability question. But one thing that seems very likely in terms of theory and some kinds of data I have seen is that the simple-minded homogeneous solutions to a lot of these problems are not the most likely ones, and this is a hairy problem to handle theoretically. I think in many cases these are unstable, and when you start encountering these hills and valleys, the specimen will give a little one place and slack off another. Experimentally, what happens is that when you get an ultraslow loading rate, what little data I have seen suggests that (1) you have a fairly homogeneous state of strain in tension, compression, or torsion (and the data I'm talking about here is primarily torsion), (2) if you go up to rates of loading which would be slow enough so that elastically you would think it perfectly homogeneous--the time for many of the stress waves to go back and forth--that the situation is quite inhomogeneous, (3) a strain sort of develops here and there on the specimen, and it takes a long time to settle down, and (4) if your loading rate is not too slow, they can have time to settle down before another configuration develops. Therefore, all I can really say is that the problem appears to be complicated.

WINN, GENERAL ELECTRIC

It's kind of hard to follow mathematical argument in brief fashion, so I'll restrict my thoughts to a physical question. If I followed your arguments correctly, you have added the rotational terms and the rotational momentum and energy balance for the individual material particles through the usual treatment. Now, have you made any correlation between the addition of these terms and the gross mechanical behavior resulting therefrom so that you can deduce certain gross behavior from the partition of energy between the individual modes? In other words, the energy and momentum balance is usually taken from the translation of modes. Did you add the momentum and energy balance for the material particles in its rotational modes?

DR. ERICKSEN

In a manner of speaking. I think the difficulty may come from talking about particles because this is usually in a different sense. Now, when you're likely to encounter a theory of this general kind, one should be careful not to confuse particles with atoms. That's what you are doing in terms of atomic theory, averaging over regions which involve a large number of atoms, and it's sort of an average of those regions that plays the role of the continuing particle. Now, with respect to other possibilities in this direction, this is a problem which really hasn't been checked out. Liquid crystal theory is a clearer situation. What you have is a long molecule with many atoms which you treat as an entity. Then these terms would arise from the fact that (if you don't want to think energetically) you have translational energy associated with the center of mass along a rod; you also can have rotational kinetic energy with the center mass just associated with spinning a rod around. This sort of thing is part of what is involved in these momentum statements and the general balances that are treated here. I don't know if this is at all comprehensive.

DR. DRUCKER

I think a short answer to the question was "Yes." We will now adjourn for a coffee break.

SYMPOSIUM RECONVENES

COLONEL STANDIFER

Would everyone please come in and take their seats quickly so that each of the speakers may have his full time? I have an idea that perhaps we should divide into teams, put the physicists at one set of tables and the metallurgists at another, the mechanical at another and the aircraft structures people at another, so we can identify the tone of the questions that are coming up. If we don't do anything else, identify the possibility of our coming up with a common language. I'll turn the mike over to the session chairman.

DR. DRUCKER

I didn't hear all the remarks, but I did hear something about common language. I hope indeed no one indulges in common language. (Laughter.)

Well, we have gone from a scale with very complicated physical backgrounds to a complicated physical background with a tensor, this third order that we saw on the board. So I think we have covered the range of the complexities in the sense that one can expect. Now, we will hear about the metallurgical side from Dr. Stroh who, of course, has made many contributions in the area of dislocation theory and allied topics. Then, as a final subject of this morning's session, a more conventional applied mechanics problem will be described to you.

Our next speaker on the program is Dr. Allen N. Stroh, who received his Ph.D. degree from Bristol, which you know has been the center of dislocations activity for a long time. He will speak to us on Defects in Solids at High Velocities. Dr. Stroh.

DR. ALLEN N. STROH

I'm to concentrate my remarks on the particular defect, the dislocation. This is the most relevant to the conference because it is highly mobile and therefore should show velocity dependent effects. In particular, I should like to discuss the factors, or some of the factors which will limit the dislocation velocity. Well, it's often been stated that the velocity of sound forms an absolute limit of that past which no dislocation can move. This is a topic which I think needs re-examination; but first look at a case where this would be an important restriction. I'd like to discuss a model which I think Cyril Smith first gave of the structure of a shock front in a crystal.

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DEFECTS IN SOLIDS AT HIGH VELOCITY DEFORMATION

by

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DEFECTS IN SOLIDS AT HIGH VELOCITY DEFORMATION

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ABSTRACT

The behavior of dislocations moving at both subsonic and supersonic velocities is reviewed. At supersonic velocities energy is radiated and a rather high stress is needed to maintain the motion. The properties of edge and screw dislocations which differ much more at high velocities than at low are contrasted. A major problem of dislocation dynamics is the nature of the resistive forces limiting the velocity and current ideas on this are discussed. The dependence of the dislocation velocity on the applied stress has been measured for several materials, but the relationship is not yet fully understood. Finally the production of point defects by rapidly moving dislocations is considered.

DEFECTS IN SOLIDS AT HIGH VELOCITY DEFORMATION

1. INTRODUCTION

In considering the properties of crystal defects under high velocity deformation it is inevitable that we give most of our attention to one particular defect -- the dislocation. For not only is this directly associated with the process of plastic deformation, but it is also highly mobile so that its dynamical properties are likely to be of importance. Point defects -- vacancies, interstial atoms and impurities -- on the other hand are essentially slowly moving entities and there is no reason to suppose that they will behave differently at high strain rates than at low. It is true that rapidly moving atoms can be introduced in the material by nuclear radiation but this is a separate field whose study would take us rather far from the subject of plastic deformation. Finally surface defects -- twin boundaries, crystals interfaces, etc. -- can at least formally be analysed into arrays of dislocations and so a study of the latter is basic to an understanding of their behaviour.

In recent years much progress has been made in the direct experimental observation of dislocations. Most of this work, however, shows the dislocation arrangements at one particular instant of time, and rarely gives any quantitative information on their dynamical behaviour. However, there is a small but important set of experiments which will be discussed later, relating the dislocation velocity to the applied stress. Some information on this topic can also be gained from internal friction measurements, though by their nature these are limited to very low dislocation velocities. Apart from this the subject must be studied by purely theoretical methods.

In general the questions we ask about moving dislocations fall into one or other of two classes. In the simpler class of questions (c.f. sections 3-5) we assume the dislocation motion to be known, for example the dislocation is moving with constant velocity, and ask what is the stress field, or the energy, or some such property associated with it. In the second and more difficult class (sections 6-9) we consider a dislocation in a given environment and ask what its motion will be. While the first can fairly confidently be answered, the second are at best only imperfectly understood.

2. DISLOCATION BOUNDARIES AND SHOCK WAVES

In a plane shock wave the primary effect is a uniaxial compression normal to the shock front; since this results in high shear stresses,

plastic flow occurs to convert it into a hydrostatic compression. But then the lattice spacing changes across the shock front so that an array of dislocations must be formed. The simplest array which can accommodate the change in structure and also move in the direction of the shock propagation is that shown in figure 1 as suggested by Smith.[1] If the dislocations are to maintain their positions in the shock front they must, since their glide planes are at an angle of 45° to the direction of propagation, move with a velocity 40% greater than that of the shock wave which itself may have a velocity greater than the velocity of sound in the material. It will therefore be a matter of interest to consider whether supersonic dislocations are possible; we shall discuss this in the following sections.

3. UNIFORMLY MOVING SCREW DISLOCATIONS

The problem of a dislocation moving with constant velocity has been studied for both isotropic and anisotropic materials by a number of authors[2]-[8]; to avoid purely mathematical complexities which shed little new light on the physical ideas involved we shall consider here only isotropic materials. The simplest case is that of a uniformly moving screw dislocation; here the displacement w is parallel to the dislocation line (the z axis) and the elastic equations reduce to the single equation

$$\partial^2 w/\partial x^2 + \partial^2 w/\partial y^2 = c_2^{-2}\, \partial^2 w/\partial t^2, \tag{1}$$

where c_2 is the velocity of transverse waves in the medium. For a dislocation at rest the right hand side of eqn. (1) is zero and we obtain

$$w = b\Theta/2\pi = (b/2\pi)\tan^{-1}(y/x), \tag{2}$$

giving an increase in the displacement by an amount b, the Burgersvector, each time we encircle the dislocation. If the dislocation is moving with constant velocity V in the x direction $(V < c_2)$ the corresponding solution of equation (1) is[2] with $x' = x - Vt$,

$$w = (b/2\pi)\tan^{-1}(\beta y/x'), \tag{3}$$

where $\beta = (1 - V^2/c_2^2)^{1/2}$ (4)

At low velocities there is little change in the strain pattern, but as V increases the whole strain field is contracted in the x direction and the deformation is more and more concentrated in the plane $x' = 0$ transverse to the motion.

In the limit when $V = c_2$, w is constant except on the plane $x' = 0$ where it changes abruptly by an amount ½b (so that the total change in w on encircling the dislocation remains equal to b). This discontinuity across the plane $x' = 0$ represents a wave travelling with the dislocation at the velocity c_2. At velocities $V > c_2$ the dislocation is supersonic; the waves generated must propagate with velocity c_2 and will therefore trail behind the dislocation at the Mach angle $\alpha = \sin^{-1} c_2/V$ (figure 2); w will change by ½b on these waves and will be constant elsewhere.

Thus at any velocity V, subsonic or supersonic, we have found displacements satisfying the elastic equations and representing a moving screw dislocation. In contradiction to this it has sometimes been stated that the velocity c_2 constitutes a limit to the dislocation velocity; this arguement is based on a consideration of the energy of a moving dislocation. From equation (3), we find the total energy (strain energy plus kinetic energy) of the moving dislocation is

$$E = E_0/\beta, \qquad (5)$$

where E_0 is the rest energy; this becomes infinite (c.f. equation 4) as the velocity c_2 is approached. But we have seen that the displacement becomes discontinuous at c_2 and it is then impossible to compute the strain energy from linear elastic theory; a calculation based on an atomic model would clearly give a finite result. Hence the infinity in energy given by eqn. (5) merely represents an inadequacy in linear elastic theory and not an absolute limitation on the dislocation velocity.

It is still, however, an open question whether conditions exist under which dislocations actually do become supersonic; because energy is radiated, a high stress is needed to maintain the motion and such a stress in unlikely to occur except possibly under conditions of high impact loading. A rather crude estimate of the stress involved can be made if we assume that the displacement in the wave occurs between two adjacent atomic planes separated a distance a across which a displacement ξ produces a sinusoidal restoring force $(\mu b/2\pi a)\sin(2\pi\xi/b)$ per unit area. Then the energy per unit area of the waves is $\mu b^2/2\pi^2 a$ and to generate this energy a force σb per unit length of the dislocation is needed where

$$\sigma = (\mu b/\pi^2 a)\cos\alpha = (\mu b/\pi^2 a)(1 - c_2^2/V^2)^{1/2}. \qquad (6)$$

Unless the dislocation is only just supersonic, this stress is comparable to (but less than) the ideal strength of the material which, again on a sinusoidal law, is $\mu b/2\pi a$.

If we cease to regard the material as an elastic continuum but take into account the discrete atomic structure, we find that the medium is dispersive, waves with wavelength comparable to the atomic spacing propagating at a velocity less than c_2, the velocity of the long waves. Accordingly supersonic effects start earlier than continuum theory predicts and high frequency waves are generated at velocities a little less than c_2. Eshelby[9] has investigated this, but find the retarding forces depends very sensitively on the details of the structure of the dislocation core since this determines the relative amount of short frequency components present in the displacement.

4. UNIFORMLY MOVING EDGE DISLOCATIONS

Similar considerations will also apply to an edge dislocation moving with constant velocity. Certain additional effects are found, however,

which we must consider. Since an edge produces both dilational and sheer strains, the velocity c_1 of the longitudinal waves as well as that of the transverse waves, c_2, will be involved. Corresponding to equation (3) we have the displacements[3] for $V < c_2$

$$u = (b c_2^2/\pi V^2)\left[\tan^{-1}(\gamma y/x') - \tfrac{1}{2}(1+\beta^2)\tan^{-1}(\beta y/x')\right],$$
$$v = (b c_2^2/\pi V^2)\left[\gamma \log(x'^2+\gamma^2 y^2)^{1/2} - \tfrac{1}{2}(1+\beta^2)\beta^{-1}\log(x'^2+\beta^2 y^2)^{1/2}\right], \quad (7)$$

where β is defined as before (eqn. 4) and

$$\gamma = (1 - V^2/c_1^2)^{1/2}. \quad (8)$$

At the velocity c_2 a transverse wave will be generated. For velocities $c_2 < V < c_1$, we must omit the second terms in brackets in eqns.(7) and substitute a pair of waves trailing behind the dislocation at the Mach angle $\alpha_2 = \sin^{-1} c_2/V$; across these waves there is a transverse discontinuity in displacement which may be shown[7] to be

$$u_2 = \tfrac{1}{2} b\,(V^2 - 2c_2^2)/(V^2 - c_2^2)^{1/2} V \quad (9)$$

Finally if $V > c_1$, we have two pairs of waves only; the pair just considered and a pair at the Mach angle $\alpha_1 = \sin^{-1} c_1/V$, across which there is normal discontinuity in displacement of

$$u_1 = b c_2^2/c_1 V \quad (10)$$

Examination of eqns. (7) or (9) shows that at the velocity $V = c_2$ the displacement becomes infinite. Closer consideration shows that this is a resonance phenomenon which arises because at this velocity the dislocation (which acts like a driving force) moves in phase with one of the natural modes of oscillation of the medium, the shear wave. (The corresponding resonances which might be expected for a screw at velocity c_2 or an edge at velocity c_1 happen not to be excited). Our familiarity with other cases of resonance suggests that the infinities will be eliminated when either higher order (i.e. non-linear) terms or dissipation effects are included. Thus again the occurrence of infinite terms in our equations does not represent an absolute limit on the dislocation velocity. However the fact that near the velocity c_2 the displacements become large for an edge but not for a screw may be of significance, and leads to a rather different variation of the energy with velocity for the two types of dislocation; for an edge the dominant term in the energy, as calculated from eqn. (7), is $E_0\beta^{-3}$ and so at high velocities is large compared with the value $E_0\beta^{-1}$ for a screw.

Eqn. (9) shows that the amplitude of the wave decreases with increasing velocity from a large value near $V = c_2$ to zero at $V = \sqrt{2}\,c_2$; in this range the rate of radiation of energy and so the force needed to maintain the motion will also decrease with increasing velocity. Thus (assuming other sources of energy loss are not sufficient to reverse this trend) motion with constant velocity will be unstable and a dislocation which attains the velocity c_2 should then accelerate to a velocity of at least $\sqrt{2}\ c_2$.

Clearly when the discrete atomic structure is taken into account the displacements of the waves will be spread over a distance d of a few

atomic spacings whose magnitude will be related to the width of the dislocation.[9] Now d must increase with increasing wave amplitude so that at resonance the dislocation will be very wide. For the wave will take time $t = d/c_2$ to cross each particle and in this time material having mass ρd per unit area of the wave undergoes a displacement u; but, if σ_m is the ideal strength, the maximum acceleration which can be produced is $f = \sigma_m/\rho d$, and writing $u = \frac{1}{2} f t^2$ we obtain

$$d = 2\mu u/\sigma_m, \tag{11}$$

showing that d is large when u is.

A peculiarity of fast subsonic dislocations which Weertman [10] has particularly emphasized appears when we consider the shear stress produced by the dislocation on its own glide plane. From equation (7) this is

$$\sigma_{xy} = \mu(\partial u/\partial y + \partial v/\partial x)_{y=0} = (\mu b c_2^2/2\pi v^2)[4\gamma - (1+\beta^2)^2/\beta] \tag{12}$$

At the velocity c_R for which

$$4\beta\gamma - (1+\beta^2)^2 = 0 \tag{13}$$

the stress σ_{xy} changes sign. Since this is the component of the stress which determines the interaction of the dislocation with a similar one in the same glide plane, at velocities greater than c_R like dislocations will attract one another instead of repelling; the velocity c_R given by eqn. (13) is the velocity of the Rayleigh waves.

As an example of the above, consider the case of a screw dislocation in a close packed lattice; the dislocation dissociated into partials which separate to a distance given by a balance between the repulsion of the partials and the tendency of the stacking fault between them to contract. At slow speeds the elastic interaction is less than at rest, and so, if we assume the stacking fault energy is unchanged, the extended dislocation will contract; at the velocity c_R it is about 70% of its original width. However very near to c_2 the interaction of the edge components must dominate owing to the presence of the large factor $1/\beta$ (c.f. eqn. 12); since the two edge components are of opposite sign they repel one another and the dislocation becomes very wide. Nevertheless the contraction at lower speeds is probably the more important effect since it occurs over a wider and more accessible range of velocities. It should result in a moving screw cross-slipping more easily than a stationary one.

5. NON-UNIFORM MOTION

Eshelby[11] has treated the problem of a screw moving in an arbitrary, but always subsonic, manner. If the motion is prescribed the stress field can, in principle, always be found but the stresses at any point depend on the whole history of the dislocation's motion. The problem is therefore rather more complicated than the analogous one of a moving point change. Quite generally, an accelerated dislocation will radiate energy, but only

if the motion of the dislocation is confined to a finite region can the radiation field be clearly distinguished. An example is the oscillating screw which radiates energy at a rate proportional to the cube of the frequency.[12] Recently Kosevich[13] has shown that the radiation from an arbitrary system of dislocation loops can be treated in a manner analogous to the radiation from an electric dipole.

6. THE EQUATION OF MOTION OF A DISLOCATION

Familiarity with classical mechanics suggests writing the equation of motion of a dislocation in the form

Force = mass x acceleration,

and we shall consider now to what extent this is valid. The idea of a force on the dislocation gives no difficulty, and the force is σb per unit length where σ is the resolved shear stress. Earlier suggestions that there is in addition a velocity dependent force, analogous to the Lorentz force of electro-dynamics, do not now appear to be justified.[14] [15]

On the other hand the mass of a dislocation is not a very well defined quantity. The simplest approach is to write eqn.(5) for the energy of a moving screw in the form

$$E = E_o + \tfrac{1}{2} E_o (V/c)^2 + \cdots,$$

valid for small V, and to identify the coefficient of $\tfrac{1}{2}V^2$ with the mass. Since

$$E_o = (\mu b^2/4\pi) \log(R/b), \tag{14}$$

where R measures the distance to which the stress field of the dislocation extends, we obtain for the mass per unit length

$$m = (\rho b^2/4\pi) \log(R/b), \tag{15}$$

with ρ the density of the material. An exact solution of some special cases of motion under known forces[11] confirms the form (15); the parameter R however depends on the whole history of the dislocation's motion. For consider a dislocation which was at rest until time $t=0$, and then began to move under an applied stress. At time t no effect of the motion can extend further from the dislocation than a distance c_2t and so this is the appropriate value of R; the "mass" of the dislocation then increases with time.

Nevertheless, because R enters into m only in the argument of a logarithm, the value we obtain for the mass will not be very sensitive to the exact value of R provided it does not vary in order of magnitude. To obtain simple though approximate results to problems of dislocation motion it is often convenient to introduce a constant mass of the form (15). This has the same sort of validity as the more familiar approximation of assigning a constant line tension to a dislocation. For edge dislocations (10) (15) should be multiplied by the factor $(1 + c_2^4/c_1^4)$

7. RESISTANCE TO THE MOTION

A factor of major importance to the motion of a dislocation is the nature of the resistive force to opposing the motion, which will determine the final steady velocity achieved under a given applied stress. Experimentally this relation has been measured by observing the dislocation position by etch pitting before and after the application of a stress pulse of known duration. In this way results have been obtained for lithium fluoride,(16) (figures 3 and 4) for silicon iron(14)(fig. 5) and for germanium and silicon.(18) (19) For lithium fluoride and silicon iron (14) (fig. 5) and for germanium and silicon(18) (19). For lithium fluoride and silicon iron the experimental results can be represented by the equation

$$V = V_0 e^{-A/\sigma} \tag{16}$$

where V_0 and A are constants. In silicon and germanium the velocity varies less rapidly with stress being proportioned to σ^n with n between 1 and 2. In all cases the temperature variation is, at least approximately, that expected if an activation energy is involved.

The presence of other defects in the crystal is likely to influence the dislocation motion and indeed radiation of the crystal (so as to increase the number of point defects) is found to increase the resistance to the dislocation motion. However, this aspect of the problem has as yet received little attention and we shall therefore ignore the possible effects of other imperfections. The mechanism of dislocation damping which has been most fully investigated is the interaction with phonons which we will consider in the next section; however it appears unlikely that this can account for the experimental results referred to above. A more promising mechanism is the action of a Peierls force leading to the propagation of kinks along the dislocation; this will be discussed in section 9.

8. INTERACTION OF A DISLOCATION WITH PHONONS

Leibfried (20) pointed out that a dislocation, being a region of plastic strain, can scatter phonons, and that as it moved through the crystal more phonons would be incident on, and so scattered from, the leading side than on the trailing side; this imbalance gives a net force opposing the motion which he estimated to be

$$\sigma b \simeq v d \bar{E} / 10 c_2, \tag{17}$$

where $\bar{E}$ is the phonon energy density and d is the scattering width (i.e. scattering cross-section per unit length) of the dislocation.

The main problem is to find the scattering width d. Originally it was assumed that $d \sim b$ but more recent calculations (21)(22) give for edge dislocations

$$d \sim (\gamma^2 b^2/3\lambda) \log(R/b), \tag{18}$$

where γ is Grüniesen's constant and λ the phonon wave length; R as usual measures the distance the stress field of the dislocation extends. For screw dislocations the logarithmic factor should be omitted. Experimental values of the scattering width can be found from the effect of dislocations on the thermal conductivity of the material. In this way Sproull et al[23] have found $d = 2.5 \times 10^{-6} T$ for lithium fluoride at a temperature of T °K (The temperature factor arises because the mean phonon wave length changes with temperature; it is useful in interpreting the experimental results as it distinguishes the contribution of the dislocations from other sources of scattering.) This scattering width is greater, by a factor of 2 or 3, than that calculated from equation (18); the difference, however, is understandable if some of the dislocations are separated by distances less than the phonon wave length, (e.g. small piled up groups) for then the scattering from them will be coherent and the apparent scattering width will be increased.

We have tacitly assumed that in the scattering the phonons can be treated independently; this will be justified if the phonon mean free path is sufficiently large compared with the scattering width. If this is not so, it is better to regard the process as a diffusion of phonons round the dislocation; this has been done by Eshelby[12], and again there results a resistive force proportional to the dislocation velocity.

So far the dislocation has been treated as if it were rigidly constrained; however Nabarro [24] pointed out that the thermal waves (phonons) will cause the dislocation to oscillate. Since an oscillating dislocation radiates energy (Section 5) this would give another mechanism for phonon scattering, whose scattering width should be rather greater than that considered above. Nabarro had thought that the effect on the damping of a moving dislocation would vanish to order V/c_{ι}, but recent studies[25] [26] suggest that this is not so. However this contribution to the scattering is not observed in thermal conductivity measurements, presumably because the Peierls force is sufficient to prevent the oscillation.

It has also been suggested[27] that as the dislocation moves the periodic structure of the crystal will impose an oscillation on the dislocation and so cause it to radiate; however the magnitude of this effect appears to be rather small.[28]

None of the processes we have considered leads to the empirical relation (16); nor does the predicted temperature variation correspond to that observed, and generally the magnitude of the damping calculated is too small. We must conclude therefore that the interaction with phonons is not the main factor in the resistance to dislocation motion. On the other hand it has been claimed [29] that internal friction measurements are in accord with dislocation damping by phonons; the results however are not unambiguous.[25]

9. KINKS

If the Peierls force, i.e. the influence of the periodic lattice structure on a dislocation, is sufficiently large the dislocation may become trapped along a close packed atomic row. To move, the dislocation must then throw a segment from one close packed row to an adjacent one over the potential hill between them, forming a pair of kinks (fig. 6); the kinks can then propagate along the length of the dislocation until the whole dislocation has advanced one atomic spacing. The most difficult stage of this process will be the formation of the pair of kinks which will require an activation energy; the subsequent propagation of the kinks will be relatively easy. This is essentially the process which Seeger (30) (31) has proposed to explain the Bordoni peaks observed in the internal friction of certain metals at low temperatures.[(32) (33)]

The activation energy required has been studied by a number of workers (25)(30)(31)(34) with the application to internal friction in mind; however a rather different treatment due to Gillman [(35)] and based on a calculation of Fisher's [(36)] better reproduces the experimental result expressed in eqn.(16). If a length AB of dislocation breaks away, the energy is increased both because the total length of the dislocation has increased, and because the energy per unit length has increased, but work is done by the applied stress in increasing the area of slip; as the loop grows its energy passes through a maximum which will be the activation energy, W. A simple calculation gives

$$W = 2^{3/2} E^{1/2} \Delta E^{3/2} / 3\sigma b, \tag{19}$$

where E is the line tension, and ΔE the increase in energy per unit length on lifting the dislocation from its potential valley (i.e. the Peierls energy). Then since this process results in the dislocation's moving forward a distance b, its velocity is

$$v = \nu b \exp[-2^{3/2} E^{1/2} \Delta E^{3/2} / 3\sigma b k T], \tag{20}$$

with ν an atomic frequency of vibration. This is of the form observed; in addition the numerical values seem reasonable for lithium fluoride. Thus on comparing with the experimental values we obtain $\nu = 3 \cdot 5 \times 10^{12}$ sec^{-1} and $\Delta E = 10^{-3} E$. Also at a stress of $1 \cdot 1 \times 10^{8}$ dynes/cm^2 the measured activation energy is 0.7 e.v. compared with a calculated value of 0.4 e.v.; considering the very approximate nature of the calculations involved this is not unsatisfactory.

For silicon iron the agreement is not quite so good; in particular the pre-exponential factor V_0 is found to vary with temperature (fig. 5).

It is implicit in the above calculation that the Peierls energy is small so that the dislocation configuration is mainly controlled by the line tension; for lithium fluoride this is justified by the small value found for ΔE. In a covalent crystal, however, a displacement of the

dislocation will involve the breaking and reforming of valence bonds and we expect the Peierls energy to be large; then the kinks will be narrow (rather as shown in fig. 6) so as to minimise the length of dislocation lying between close packed rows. The critical position will be when the kinks are one atomic spacing apart so that their formation involves breaking only one bond (energy W) and the activation energy is $W - \sigma b^3$, where σb^3 represents the work done by the applied stress. Taking the difference between forward and backward flows we find the dislocation velocity

$$v = (b^4 \sigma \gamma / kT) \exp(-W/kT) \tag{21}$$

The experimental value of the activation energy, 1.9 e.v. for germanium, is in good agreement with the bond energy of 2.0 e.v., but the observed stress dependence is nearer the second power than the first.

We have assumed that the dislocation motion is governed by the difficulty of formation of kinks and not by their mobility but unless the kink density is very large, the velocity of kinks along the dislocation line will be much greater than the dislocation velocity. At even moderate dislocation velocities, the kink velocity will be high. There will then be a resistance to the motion of the kinks due to the scattering of photons (26) and, if the kink should become supersonic, to the radiation of energy. Such effects will need to be taken into account in a complete treatment of the dislocation motion.

10. GENERATION OF POINT DEFECTS

A jog in a screw dislocation can only move with the dislocation if it leaves behind it a trail of vacancies or interstial atoms. It can avoid this by slipping along the length of the dislocation instead. However just as in the case of kinks this sideways motion becomes more and more difficult as the dislocation velocity is increased, suggesting that the jog may be forced to move with the dislocation generating more point defects at high velocities. On the other hand at high strain rates there is little time for point defects to diffuse and thus take part in the deformation process so that the number formed may not be of great significance.

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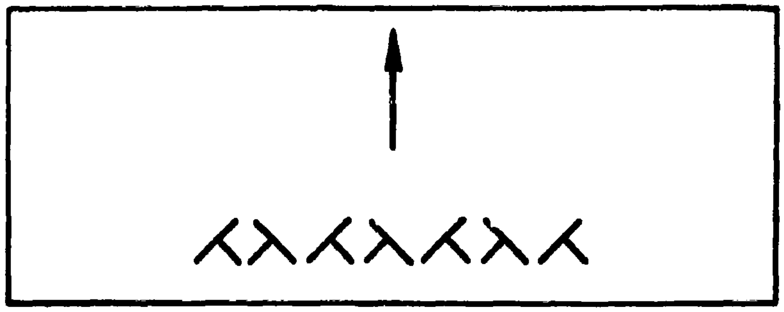

FIGURE 1. A SIMPLE ARRAY OF LATTICE SPACINGS

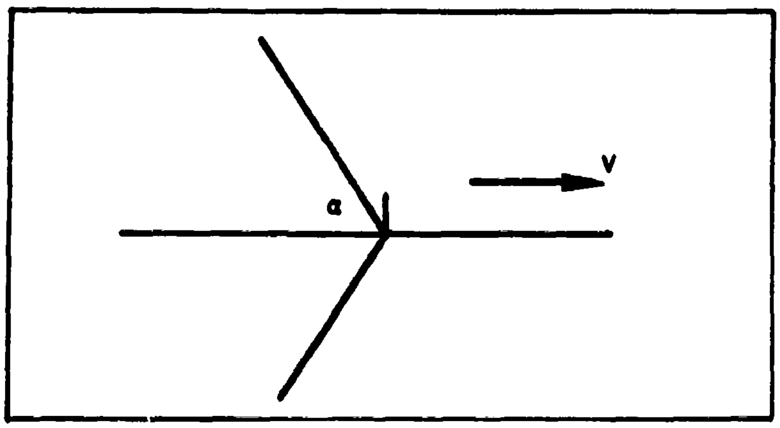

FIGURE 2. MACH ANGLE $\alpha = \sin^{-1} \frac{C_2}{V}$

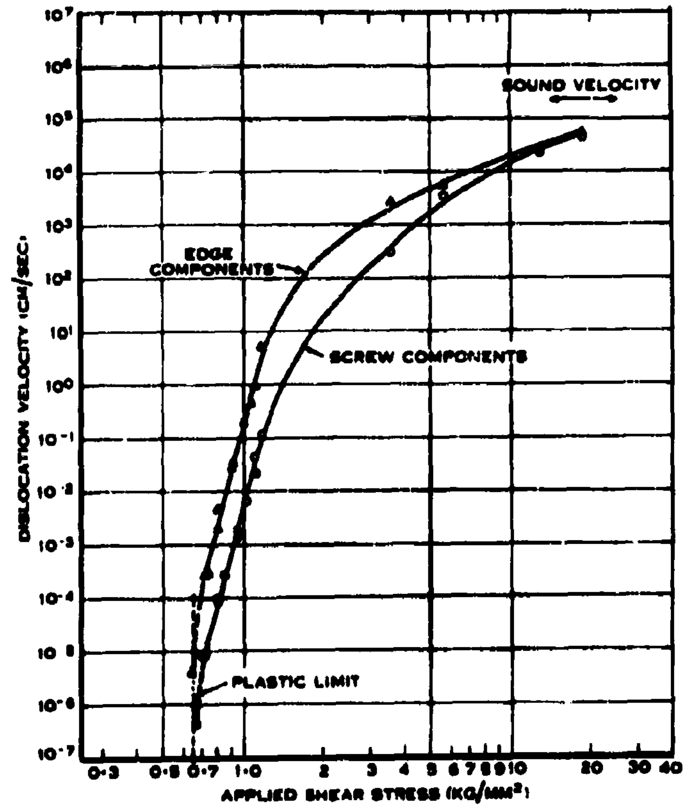

FIGURE 3. DISLOCATION VELOCITIES ACHIEVED UNDER APPLIED STRESS

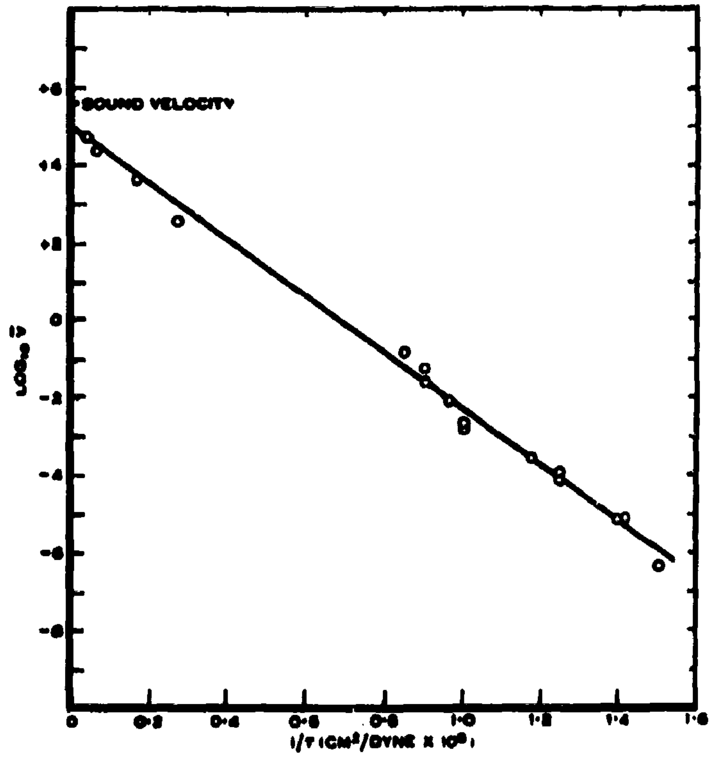

FIGURE 4. DISLOCATION VELOCITIES ACHIEVED UNDER APPLIED STRESS

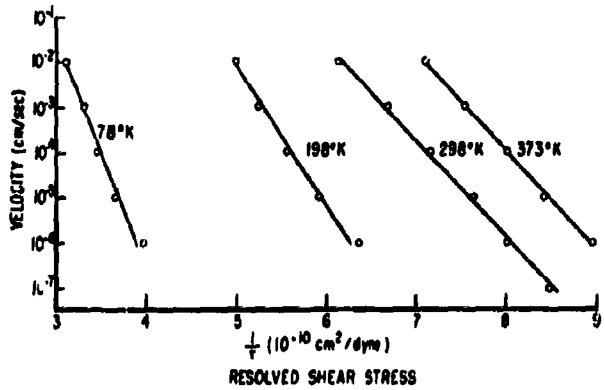

FIGURE 5. DISLOCATION VELOCITIES ACHIEVED UNDER APPLIED STRESS

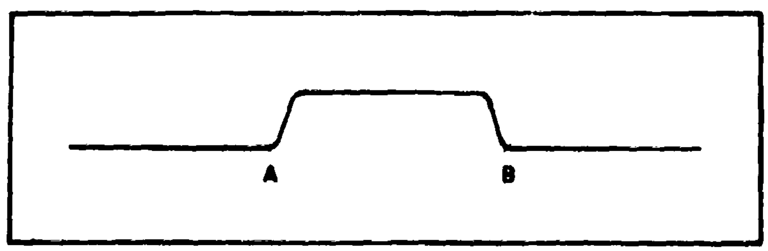

FIGURE 6. DISLOCATION ENERGY

DISCUSSION

DR. DRUCKER

The presentation by Dr. Stroh is now open for discussion.

DR. HERRMANN, MIT

I'd just like a point of information. Some of the troubles, the singularities with the dislocation solutions really result from using Voltair dislocations. Has anyone ever considered the possibility of using anything but Voltair dislocations? We are having difficulty in smoothing out the discontinuities with respect to this question of dislocations moving around sonic speeds.

DR. STROH

A model of finite width must be used which, like you say, would be one for proving the basis of your mentioning that there would be some radiation starting below the subsonic velocities because of the dispersive effects. The difficulty, of course, with the other theory is that one must put in a structure, one has to determine what the dislocation is, and this is a rather complicated thing to do. It's something which ought to be done, but at the moment not much progress has been made. People have used the static case to find the size force, and have come up with different calculations whose results differ by several orders of magnitude.

FROM THE FLOOR

If I interpret correctly, you run across more or less the same problem as you do with common calculations. Is this correct?

DR. STROH

Yes, I'd say that is correct.

DR. BRODE

At the risk of displaying some considerable ignorance about dislocations, I'm interested in what you find in the way of actual evidence. When you consider a shock wave, you have these propagating dislocations essentially higher than their natural velocities, sonic velocities. You suggest this is real. I wonder what the evidence is for this. It seems to me that the shock wave would present essentially a generating mechanism which from one point to another is noncausal as far as any single dislocation is concerned. Therefore, I wonder if the whole concept of a propagating dislocation is valid. I listened carefully but I didn't understand that this was really observed, that a single dislocation is something you can associate with two different points which have been connected by a pass to the shock wave.

DR. STROH

Yes. If one has a shock wave coming, one can have this front there and another one there, and one has a similar situation. You can either have generated this new edge point along the way, or you can say that this moves with the shock front. As far as I know, experimental evidence is not sufficient to decide between them. All I would actually argue is that one should bear both possibilities in mind and not rule out our theory that there may be some dislocation motion following the shock front.

DR. BRODE

The point being, it appears to me that it is not a useful concept if it's noncausal because you consider these to be connected. It's like an axe blade striking a piece of wood. You might say that the split in the wood propagates as the blade punches through the top of the wood, but that's not very useful really. It's that the blade is applied locally at each point and whether it strikes at an angle or not the wood is split. It's like hitting a wedge to the shock--and look at the propagation along the surface of a disturbance, and that goes extremely fast depending on what the angle to the shock is. It has no real causal relation.

DR. STROH

Well, I think there is a physical distinction.

DR. BRODE

That's what I'm asking! Do you find that there is a physical reality to this notion of propagating structures, and has this been encountered? Is it observed? Do you find single dislocations? Can you observe them?

DR. STROH

Not so far; none has been observed as yet. I think it would be very fine if one could happen experimentally, but I would insist that there is a physical meaning to the question whether one starts with a dislocation here and moves it along with this shock front, or whether one has a shock front which regenerates new dislocations at successive points along its motion. I think these are distinct processes which can really only be settled by experiment.

DR. BRODE

When you say that the probability is that the shock front does generate dislocations out front of the shock front, the probability is that there are no dislocations.

DR. STROH

Well--I don't know.

DR. DRUCKER

I'm sorry I have to cut this discussion short, but we must move on to the next speaker. Thank you, Professor Stroh.

The next paper on the program has two authors. Dr. Williams and Brother Arenz, both of California Institute of Technology. The paper will be presented by Brother Arenz. The title of his paper is "Dynamic Analysis in Viscoelastic Media." Brother Arenz.

BROTHER R. J. ARENZ

I imagine that most of you have at one time or another seen an interesting little material known as crazy putty. A piece of it can be drawn out slowly in your hands and it will relax and sag, or you can throw it rather violently on the ground and the material will shatter. These are some indications of a property which we call viscoelasticity. Indeed a viscoelastic material has as its distinguishing characteristic the fact that it is strain sensitive to both rate and temperature effects, and that is in addition to the effect of any time parameter that might enter into dynamic analysis due to wave propagation or inertial effects.

DYNAMIC ANALYSIS IN VISCOELASTIC MEDIA

by

M. L. Williams, Ph.D. and R. J. Arenz

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DYNAMIC ANALYSIS IN VISCOELASTIC MEDIA

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ABSTRACT

Distinguishing characteristics of viscoelastic media are reviewed with special reference to dynamic stress analysis. To circumvent the inherent computational difficulties in the usual transform type of solutions, an extension of the Schapery method is proposed for approximating the viscoelastic strain distribution due to wave effects. From an experimental standpoint, the use of photoelastic materials to model the responses due to dynamic loading is discussed. It is emphasized that quantitative analysis depends upon knowing the birefringence as a function of strain rate and temperature. The association of the stress and strain optic coefficients to mechanical properties is derived and suggestions are made as to the determination of material characterization as a function of reduced strain rate.

DYNAMIC ANALYSIS IN VISCOELASTIC MEDIA

INTRODUCTION

The distinguishing characteristic of viscoelastic material is its strong sensitivity to rate and temperature effects. Hence in addition to the usual effect of the time parameter as reflected in vibration or stress wave phenomena, there arises the possibility of direct interaction between loading input and basic material properties. As discussed by Hunter (1) in a review of this subject in 1960, this interplay leads to analytical complications which have so far been resolved only in simple geometries or simple mathematical characterizations of the viscoelastic material in question. The purpose of this paper is to present an approximate analysis for linearly viscoelastic stress wave phenomena, and to present some further observations upon how the analytical results may be checked experimentally using photoviscoelastic techniques. It is expected that future judicious combinations of such analysis and experiment will permit a more efficient study of engineering situations.

SECTION I: GENERAL VISCOELASTIC CONSIDERATIONS

As a property of the material, the rate dependent effect is reflected in the equation of state which, in analogy with its elastic Hooke's Law form, takes an implicit form $F(\sigma, \epsilon, t, T) = 0$. Explicitly, and compatible with the results of thermodynamic analysis (2), the mathematical characterization of a linearly viscoelastic stress-strain (τ-γ) shear behavior* can be written as

$$\left[a_n(t)\frac{\partial^n}{\partial t^n}+\cdots+a_o(t)\right]\tau(x_i, t) = \left[b_m(t)\frac{\partial^m}{\partial t^m}+\cdots+b_o(t)\right]\gamma(x_i, t) \qquad (1)$$

*From a physical standpoint, linear viscoelastic behavior implies that a time dependent response due to a prescribed time dependent loading is precisely doubled (tripled, etc.) at every point in time when the prescribed input is doubled (tripled, etc.) over the same time range.

where in most applications the time dependence of the multiplicative constants arises from transient temperature changes in the material. Suppressing further consideration of temperature for the time being, consider the stress-strain relation under isothermal conditions when the a_n and b_m are constants, viz.

$$\left[a_n \frac{\partial^n}{\partial t^n} + \cdots + a_o\right] \tau = \left[b_m \frac{\partial^m}{\partial t^m} + \cdots + b_o\right] \gamma \qquad (2)$$

thus representing a series and/or parallel array of spring and dashpot elements whose spring moduli and dashpot viscosities are proportional to the constants.

When a Laplace transform is applied to Eq. (2) for zero initial conditions, there results

$$\left[a_n p^n + \cdots + a_o\right] \bar{\tau}(p) = \left[b_m p^m + \cdots + b_o\right] \bar{\gamma}(p) \qquad (3)$$

where p is the transform parameter and $\bar{\gamma}(p)$ is the Laplace transform of the function y(t). In this form, because p may be treated as an algebraic parameter, it is permissible to write Eq. (3) as

$$\bar{\tau}(p) = \left[\frac{b_m p^m + \cdots + b_o}{a_n p^n + \cdots + a_o}\right] \bar{\gamma}(p) \equiv G(p)\ \bar{\gamma}(p) \qquad (4)$$

It is immediately seen that from the formal standpoint G(p) appears to play the part of a shear modulus relating the transformed stress and strain, in a way similar to the elastic shear relation $\tau = G\gamma$. This association is the basis for the well known correspondence rule (3) by which a certain analogy has been showr to exist between elasticity and viscoelasticity problems.* This technique permits one to deduce

* It is important however to point out that inertia effects can usually be neglected in viscoelastic analyses because the ordinary viscoelastic deformations creep so slowly that the accelerations, and hence forces, associated therewith are of lower order magnitude than other stresses in the problem. On the other hand, when wave propagation is involved it is not permissible to neglect the inertia terms.

viscoelastic stresses and strains from known elastic solutions, providing as a practical matter that the Laplace inversion can be carried out.

Return now to a more careful discussion of the equation of state, in the form of a linearly viscoelastic stress-strain law. It is important to recognize that while the details of (i) measuring the applied stress, (ii) measuring the resulting time dependent strain, and, after taking their transforms, (iii) deducing the mechanical properties as reflected by G(p) in Eq. (3), are complex and in the province of the experimentalist, in principle the same procedure as in the determination of elastic mechanical properties is used. Such matters*, as well as alternate methods of mathematically characterizing the material, are treated in standard reference works (4, 5).

As one illustration of the previous point, consider the manner by which the mechanical properties are described using the complex dynamic modulus. Suppose a real sinusoidal stress, τ_o, is applied to a shear specimen (6) at a fixed frequency ω, and the resulting complex shear strain output, γ, is recorded. The latter quantity may lag the input because of the viscoelastic nature of the material and hence one has

$$\tau = \tau_o e^{i\omega t} \tag{5}$$

$$\gamma = \gamma_o e^{i(\omega t - \delta)} \tag{6}$$

In terms of the inverse of the dynamic modulus Eq. (4), i.e. the dynamic compliance k, one obtains

* Customary experiments include measuring the stress relaxation due to constant strain, creep due to constant stress, etc. The theory of linear viscoelasticity guarantees that the complete mechanical property characterization, which depends only on the materials, is unique and hence identical regardless of which test was used to determine it. This of course implies an inter-convertibility among the various tests which is useful because the accuracy in some tests is better over some ranges than others. Hence as a practical matter a combination of several tests is frequently used to completely characterize the material for the complete time range.

$$K(p) \equiv 1/G(p) = \bar{\gamma}(p)/\bar{\tau}(p) \tag{7}$$

which for $p = i\omega$ gives

$$K(\omega) = K'(\omega) - i\,K''(\omega) = \frac{\gamma_o\, e^{i(\omega t - \delta)}}{\tau_o\, e^{i\omega t}} = (\gamma_o/\tau_o)e^{-i\delta} \tag{8}$$

with

$$\gamma_o/\tau_o = \sqrt{(K')^2 + (K'')^2} \tag{9}$$

$$\delta = \tan^{-1} K''(\omega)/K'(\omega) \tag{10}$$

At a fixed frequency, an interesting quantity is the lag angle, δ which does not arise in elastic behavior. It therefore permits a convenient method of exhibiting the characteristic difference between elastic and viscoelastic behavior. Actually the two limiting values $\delta = 0$ and $\delta = \pi/2$ are points in fact. In the first case, the lag angle is zero only at high frequencies, approaching infinity. When the material is oscillating so rapidly, none of the dashpots have time to relax and hence the response depends only upon the elastic springs, and gives what is known as the glassy response. On the other hand, for a ninety degree lag, at very low frequencies approaching zero, there is so much time between complete oscillations that all the dashpots have relaxed, giving up their maximum energy, and giving again an elastic response, but one controlled by the long time or rubbery spring behavior. The lag angle therefore is a measure of the viscoelasticity in the material.

From the phenomenological standpoint, energy dissipation is another characteristic effect of viscoelasticity. In vibration problems of arbitrary dynamic input, or in wave propagation, it is evident that there will be some distribution of frequencies throughout the spectrum, each frequency contributing some lag of the strain for the imposed stress. The energy consumed during this process gives the amount of irreversible energy involved. The dissipation per unit volume, 2D, is given by the time integral of the stress in the dashpot times the strain rate, and if Eq. (5) and Eq. (6) are used, the energy per cycle becomes

$$2D' = \int_0^t \tau \,(\partial\gamma/\partial t)dt = \pi \tau_0 \gamma_0 \sin\delta \qquad (11)$$

and it is readily seen that the energy dissipation is related to the lag angle.

Indeed it is now possible to picture viscoelastic wave propagation as a phenomenon which is initiated by an initial impulse which for short times or high frequencies produces an elastic (glassy) response with no dissipation of energy ($\delta \rightarrow 0$). As the time lengthens giving more dashpots time to activate, the lag angle increases causing increasing energy dissipation until all the dashpots have completely relaxed ($\delta \rightarrow \pi/2$). It should be noted that after this point, no further relaxation is possible, and the remainder of the input passing through the medium would be controlled by the rubbery response. Inasmuch as signals pass through solids at a speed proportional to the square root of the modulus, it might be expected therefore that the first signals would propagate at a glassy speed. The strength of the front running signal would tend however to be continually killed off as the dashpots begin relaxing, and this attenuation would tend to continue until rubbery speeds, not susceptible to further attenuation, would dominate. It is found that the attenuation in viscoelastic media occurs over very short times or distances from the point of input; beyond this local region the signals travel essentially with the rubbery speed.

By way of background before pursuing the subject of viscoelastic wave propagation in detail, it is in order to give some qualitative description of the mechanical behavior of typical viscoelastic solids, as for example used in solid fuel rocket grains. The easiest properties to describe are the modulus, and ultimate stress and strain properties which are shown in Figure 1 (Ref. 7). It is seen that typical moduli run from 100 psi to 50,000 psi over 15 decades of strain rate between 10^{-5} and 10^{-10} inch/inch/minute. Ultimate uniaxial tensile strengths range from less than 25 psi at slow rates to as high as approximately 1,000 psi. The associated ultimate strains characteristically have a maximum at intermediate strain rates which range, for these filled polymers, up to 50 - 100 percent. Unfilled polymers possess basically the same qualitative behavior but of course their quantitative values differ. On these curves in addition to the strain rate, R, there appears the quantity a_T, called the time-temperature shift factor. It develops that there is an experimentally discovered (8, 9) correlation between time and temperature if the formula

$$\log_{10} a_T = \frac{-8.86(T - T_S)}{101.8 + T - T_S} \tag{12}$$

is used. T_s is a reference temperature in degrees Centigrade, usually about 50°C above the glass transition temperature. The relation Eq. (12) permits a quantitative expression of the well known qualitative similarity between high strain rate - high temperature and low strain rate - low temperature behavior.

SECTION II: WAVE PROPAGATION

Turning now to the specific problem of viscoelastic wave propagation, consider the simple geometry of a one dimensional bar loaded dynamically in compression. As usual in dynamic analysis, the equations of motion must be established and then solved. In viscoelasticity however, the latter operation is nearly always the more difficult. The Laplace transform approach noted earlier is neverthe-less suited to viscoelastic problems and forms the basis for this analysis.

It is first recalled that the material properties in elastic media can usually be considered constant, whereas such is not the case with viscoelastic solids which are rate or time dependent as pointed out in the previous discussion. Hence, in the case of wave propagation in viscoelastic media, one faces the two additional complications of representing the material properties over a fairly large number of decades of logarithmic time (or frequency of input), and then inverting the transformed equations of motion subject to these additional complications.

Information of a sort can usually be extracted if a wave front expansion and a long-time solution are employed, (10) but it is always hoped that a more complete solution is possible. One approach that has been widely used in the past ten years is the use of model represen-tation. Kolsky and Shi (11) have pointed out that most researchers have found that a realistic material representation extending over about ten decades of log time leads to a prohibitive numerical calculation problem, and as a result they have used simple spring and dashpot models with from two to four elements. These are adequate to cover one or two decades of log time but fail quite badly for larger ranges of time (or equivalently, frequency). To overcome this rather basic limitation, the present analysis provides techniques to solve certain wave propaga-tion problems using realistic broad-band viscoelastic material proper-ties. It depends on a method of material characterization utilizing a

special series representation of the operational modulus, and then a numerical inversion process by collocation to obtain the time solution of the wave propagation. As with all numerical techniques, it is more particularized than a fully analytical solution, but it is nevertheless very instructive of some rather general properties of wave propagation in viscoelastic media.

Mathematical Formulation. - To illustrate the method, the one-dimensional problem of wave propagation in a thin semi-infinite rod will be discussed. The loading on the end of the rod will consist of a step input which can be either a stress or a displacement boundary condition. Since primary interest is centered on viscoelastic effects, we assume no geometric dispersion, i.e., no lateral inertia effects will be taken into account. Indeed, it will be seen that the higher frequency components of the step loading are rapidly attenuated and consequently the geometric dispersion is less severe than it otherwise would be.

The geometry of the viscoelastic rod is shown in Figure 2. The corresponding one-dimensional equation of motion from elementary dynamics is readily written as (1)

$$\partial \sigma / \partial x = \rho \, \partial^2 u / \partial t^2 \tag{13}$$

where σ = uniaxial stress
ρ = mass density of the material
u = displacement in the x-direction.

The boundary condition imposed at the end is either

$$\sigma(o, t) = \sigma_o H(t)$$

or (14)

$$u(o, t) = u_o H(t)$$

where $H(t)$ is the Heaviside step function.

Because of the additional time dependency resulting from viscoelastic material properties, Eq. (13) can most readily be solved by taking its Laplace transform (designated in the usual way by a bar over the variable):

$$d\,\bar{\sigma}/dx = \rho\, p^2 \bar{u} \tag{15}$$

To complete the formulation of a displacement equation of motion from Eq. (15) requires a relation between the stress and displacement. The usual one-dimensional viscoelastic tensile stress-strain relationship (analogous to Eq. (2) for shear) is given by

$$P\sigma = Q\epsilon \tag{16}$$

where P and Q are linear differential operators in d^i/dt^i. Transforming Eq. (16) and using the strain-displacement relation gives

$$\bar{\sigma} = \left[Q(p)/P(p)\right]\bar{\epsilon} = \left[Q(p)/P(p)\right]\overline{du}/dx \tag{17}$$

where P and Q now become algebraic operators in the Laplace transform parameter p. If the indicated division can be carried out, the operational tensile modulus then is (compare Eq. (4))

$$E(p) = Q(p)/P(p)\,. \tag{18}$$

Combining Eqs. (17) and (18) with (15) gives

$$E(p)d^2\bar{u}/dx^2 = \rho\, p^2 \bar{u} \tag{19}$$

which has the solution

$$\bar{u}\,(p) = Ae^{-xp/c} \tag{20}$$

where

$$c(p) = \sqrt{E(p)/\rho} = \text{wave speed} \tag{21}$$

and the positive exponential has been rejected since only outgoing waves are permitted.

Standard Linear Solid Material. - In general, a large number of terms would be required in the operators P and Q of Eq. (16) for a real material; however, as pointed out above, the studies done heretofore have employed only very restricted model representations of two to four elements. Hence, in order to evaluate the method of analysis proposed here, we first consider the case of a standard linear solid to which an integral solution has been given by Morrison (12). The model

consists of three elements (Figure 3), two springs of modulus E and E', and a dashpot having a viscosity parameter μ. The stress-strain relationship (in Morrison's notation) is

$$(1/E')\, d\sigma'/dt + \mu\sigma' = (1+E/E')d\epsilon/dt + E\mu\epsilon \quad . \tag{22}$$

Thus an analytic expression is available for this simple model representation. Morrison has shown that the resulting one-dimensional differential equation of motion in terms of stress is identical in form to that for displacement; hence the solution expressions have the same form also. He derives the following transformed stress solution for a step pressure input of magnitude σ_o and the case of $E = E'$:

$$\Sigma'(\xi, p) = (1/p)\exp\left[-\xi p(1+p)^{1/2}/(1+2p)^{1/2}\right] \tag{23}$$

where $\Sigma' = \sigma'/\sigma_o$ = dimensionless stress
$\xi = \sqrt{\rho E'}\,\mu x$ = dimensionless distance
$\tau = E\mu t$ = dimensionless time

Put in terms of a time $\tau' = \tau - \xi/c_G$, the time after arrival of the fastest (glassy) wave where c_G is the glassy wave speed in the material, the transformed solution is

$$\Sigma'(\xi, p_{\tau'}) = (1/p)\exp\left\{-\xi p\left[(1+p/1+2p)^{1/2} - 1/\sqrt{2}\right]\right\} \tag{24}$$

Morrison carries out an inversion by a rather involved numerical integration of the inversion integrals to obtain the final solution for Σ'. Of course, an asymptotic expansion of $p\Sigma'$ evaluated as $p \rightarrow \infty$ gives the wave front stress.

A more widely applicable method of Laplace transform inversion to be used in this study will be briefly described and then applied to the present problem. Actually two inversion techniques are proposed; both methods have been formulated in some detail for other applications by Schapery (13). The first is a very rapid approximation technique termed the direct method; it depends on the condition that the derivative with respect to log time of the ultimate time dependent solution $(d\Sigma'(\tau')/d(\log \tau'))$ is a slowly varying function of log time. Correspondingly it is found that $d\left[p\Sigma'(p)\right]/d\left[\log p\right]$ is a slowly varying function of log p. If this condition holds over approximately a two-decade interval, then a Taylor's series expansion of the solution shows that a reasonably good approximate inversion is given by

$$\Sigma'(\tau') = \left[p\bar{\Sigma}'(p_{\tau'}) \right]_{p = 1/2\tau'} \tag{25}$$

This is an extremely rapid method to apply and as we shall see yields very informative results. The short time limit ($p \to \infty$) is recognized as giving the glassy wave front stress value.

The second and more versatile inversion method is a collocation procedure in which the transient part of the solution is represented by a Dirichlet series of decaying exponentials:

$$\Delta\Sigma'_D(\tau') = \sum_{i=1}^{n} S_i e^{-\tau'/\gamma_i} \tag{26}$$

Again without going into details, it suffices to point out heuristically that viscoelastic materials have exponential stress relaxation characteristics and the above expression ingeniously allows a wide spectrum of relaxation times, e.g., a reasonably large number of springs and dashpots may be incorporated in the material representation.

The γ_i in Eq. (26) are positive constants prescribed in such a way as to provide adequate coverage of the time spectrum and the S_i are unspecified constants to be evaluated by minimizing the total square error between the actual $\Delta\Sigma'$ and $\Delta\Sigma'_D$ given by the series. This minimizing procedure leads to the relations

$$\sum_{j=1}^{n} \frac{S_j}{1 + \frac{\gamma_i}{\gamma_j}} = \left[p\Delta\bar{\Sigma}'(p) \right]_{p = \frac{1}{\gamma_i}} \; ; \; i = 1, \; 2, \; \ldots\ldots, n \tag{27}$$

which constitute a set of simultaneous equations for n different values of p which are solved for S_i. Thus the collocation consists of a matching up of the summation on the left hand side of Eq. (27) with the calculated values of $p\Delta\bar{\Sigma}'(p)$ for various values of real, positive p. Hence the inversion procedure consists essentially in being able to determine the values of the transform all along the positive real p axis. Using irreversible thermodynamics and variational principles, Schapery has shown that all the singularities of the Laplace transform of viscoelastic stress or strain occur only on the non-positive real axis. His thermodynamic analysis was carried out for static and quasi-static situations only, so there remained a question of the validity of this representation in dynamic problems. Besides the encouraging fact that it is a least

squares procedure, the series representation $S_i e^{-\tau'/\gamma_i}$ turns out to be complete. Erdelyi (14) has shown that an infinite sequence of functions $e^{-\tau'/\gamma_i}$ is complete with respect to all quadratically integrable functions over $o < \tau' < \infty$ if the infinite series

$$\sum_{i=1}^{\infty} \gamma_i / (1+\gamma_i^2)$$

is divergent. Since we will use only a finite number of terms of the series, the divergence requirement can be satisfied by proper choice of the γ_i for large i.

A note of caution is necessary on the use of this series. Of course increased accuracy is expected by taking successively larger numbers of terms in the series. However, at the same time, commensurate accuracy in the evaluation of $p\bar{\Sigma}'(p)$ is required in order that minor deviations in the transformed values do not introduce amplified departures from the correct inverted values.

Finally then, the total time solution is given by

$$\Sigma'(\tau') = S_o + \sum_{i=1}^{n} S_i e^{-\tau'/\gamma_i} \tag{28}$$

where S_o can be evaluated by examining the behavior of $p\bar{\Sigma}'(p)$ as p tends to zero (long time solution).

Applying these inversion procedures to the problem of wave propagation in standard linear solid material gives the results shown in Figure 4 for the position $\xi = 2$. The direct method solution is of course the same curve as that of $p\bar{\Sigma}'(p)$ but is associated with the abscissa log τ' rather than log p. The collocation solution was carried out with a ten term series and is shown only as a number of computed points since it is too close to the integral solution of Morrison's to require drawing another curve.

It may be noted that the direct solution has a less steep rise than the more exact methods; this proves to be true in general for these methods. Another item of interest is the inflection point on the direct solution curve. Calculations for other positions along the rod show that the farther one goes down the rod, the closer the inflection point approaches τ'_R, the arrival time of the slowest moving or rubbery wave component of the input loading. Indeed, analytical treatment by evaluating the inflection condition $d^2(p\bar{\Sigma}') / d(\log p)^2 = 0$ shows that this is also generally true for viscoelastic rods provided the point of interest is far enough from the loaded end.

The glassy wave front is indicated in Figure 4 by the horizontal asymptote to the left at a value of $\Sigma'_G = 0.702$ which arrives at $\tau_s' = 0$ or $\tau_G = \sqrt{2}$. Also to be noted is the fact that the entire build up of the response occurs in approximately two decades of log time; this rather narrow spread in the response is the result of the somewhat fictitous simple model representation of the mechanical properties of the medium, including the fact that the low to high modulus ratio is a factor of only two (since $E = E'$).

These same response curves plotted versus actual time from initiation of loading are shown in Figure 5. While the collocation solution appears to deviate appreciably from the integral solution, the scale is large and the difference is nowhere greater than 2 percent. In addition, the integral solution was evaluated numerically and hence may be subject to some error itself. An accuracy study of the collocation method revealed that a 20 term series gave the same values as the 10 term, indicating a high degree of exactness in the results obtained.

The above comparisons indicate the usefulness and accuracy of the proposed inversion techniques. Hence we next proceed to apply them to the case of a real material.

Real Viscoelastic Material. - The specific material to be considered next is a polyurethane synthetic rubber, chosen because material data have been obtained on it (15) and it is the material used in some recent experimental wave propagation studies (16). The only new feature in this case is the representation of the material. Where before we had an analytical expression for the stress - strain relationship, now the dynamic compliance (reciprocal of modulus) in shear is given by an experimental curve (Figure 6). Noticeable immediately is the range of ten decades of log time (or equivalently log frequency) between maximum and minimum compliance values. Consequently a simple model representation as in the case of the standard linear solid will not be possible. As a matter of fact, since one spring-dashpot combination works fairly well over one decade, the desired representation might be a series or summation of such simple models, each responding predominately to a different decade of the frequency range. Indeed, general linear viscoelastic analysis (7) shows that the approximate representation is the Kelvin model (Figure 7) for which the real part of the shear compliance is given by

$$J'(\omega) = J_G + \sum_{i=1}^{n} \frac{J_i}{1+\omega^2 \tau_i^2} \tag{29}$$

where J_G = glassy (high frequency) compliance
J_i = component spring compliances
τ_i = $\eta_i J_i$ = component retardation times
η_i = component dashpot viscosities.

Thus the model is a glassy spring and n Voigt elements in series.

With the $J'(\omega)$ available from experimental data, the values of the J_i for an appropriate choice of the τ_i values can be obtained by once more using a collocating procedure at n values ω_i. It is usually sufficient to choose the τ_i at one decade intervals so that in the present case $n = 9$. Once the J_i are determined, the operational compliance required for the transformed solution is given from viscoelastic theory as

$$J(p) = J_G + \sum_{i=1}^{n} \frac{J_i}{1+p\tau_i} \tag{30}$$

For the case of a polyurethane rod, this time with a step displacement input of magnitude u_o, the transformed solution is given by

$$\frac{p\,\bar{u}(p)}{u_o} = \exp\left\{ -\frac{p\,x}{c_G} \left[\left(1+ \frac{D_{(p)} - D_G}{D_G} \right)^{1/2} - 1 \right] \right\} \tag{31}$$

where D is the tensile compliance and c_G is the glassy wave speed. For simplicity, an incompressible material has been assumed so that $D = J/3$. This assumption is realistic except for the highest frequencies which as we shall see are rapidly attenuated. The exponential in Eq. (31) can in principle be expanded in powers of $1/p$ to obtain the glassy wave front value, but it is also more easily available by the computation of $p\bar{u}(p)/u_o$ as $p \to \infty$.

The matrix inversion of the collocation solution for Eq. (31) and summation processes have been carried out on the Burroughs 220 computer; they are standard high-speed procedures and are easily accomplished. The resulting solutions are shown plotted in Figure 8. The non-dimensional response patterns at three different positions along the rod are presented; x_1, x_2 and x_3 correspond to glassy wave arrival times of 10^{-8}, 10^{-4}, and 10^{-2} seconds, respectively. As with

the standard linear solid case, the results would be identical for the stress response to a step stress input. The transform curves of $p\bar{u}(p)/u_o$ and the corresponding direct method solutions (in which p is replaced by $1/2\tau'$) are similar to those of the previous case and again we see the convergence of the inflection point and the "rubbery" arrival time, τ'_R, as x increases.

Several striking differences from the former case are evident however in the collocation solution. First is the spread of the response over about five decades of log time compared to two decades for the standard linear solid. This is a result of the inability of the simple model to represent the real viscoelastic properties of such a material as polyurethane.*

Secondly, the glassy wave front decays extremely rapidly as shown by the curve for $x_1 = 0.000167$ cm where the glassy response is $u_G/u_o = 0.09$. A wave front expansion shows this response at any point x to be given by

$$\frac{u_G(x, t')}{u_o} = H(\tau') \, e^{-\frac{x}{2c_G D_G} \sum_{i=1}^{9} \frac{D_i}{\tau_i}} \tag{32}$$

The predominant term in the summation is for i=9, and since $\tau_9 = 10^{-9}$ seconds, the relaxation is extremely fast. This corresponds to high viscosity in this element of the model and hence very rapid attenuation of the high frequency components of the input. Consequently we have also verified the earlier comment that geometric dispersion would play a less serious role in this analysis than in the case of an elastic rod. Incidentally, the limit case of purely elastic material is readily apparent from Eq. (32) since then all the τ_i would be infinite (hence exhibiting no stress relaxation nor attenuation).

A third difference is the appearance of dispersion in the response at x_2 and x_3 with the resulting oscillation in the neighborhood of τ'_R. This behavior could be predicted by considering a Fourier analysis of the step input into its sinusoidal components, each with a different propagation speed. All the components superpose to produce the input compressive displacement at x = 0 but will disperse out down the rod,

* Subsequent solution of the problem using an optimum-fit standard linear solid representation of the polyurethane material also indicates a shorter overall response time; however, some portions of the response curves in the two cases exhibit comparable slopes.

some places reinforcing and other places interfering to the extent of producing a tension. The reason this phenomenon did not appear in the standard linear solid is probably that there is only a two decade spread in its response and in addition there is not a large enough change in the modulus from low to high frequency in this representation. That there is a limit on the ultimate amplitude of the dispersive oscillations, which appear to be growing with distance down the rod, is deducible from a consideration of the Fourier series representation for the step input.

A corresponding plot of the response as a function of the total time is shown in Figure 9. While the major portion of the rise in response in the figure appears to be somewhat spread out in time due to the scale used, it actually occurs in about 0.003 seconds around τ_R .

SECTION III: DYNAMIC PHOTOVISCOELASTICITY

The final phase in this evaluation of the current status of dynamic viscoelasticity centers on experimental aspects. Since many polymers are optically birefringent, a number of investigators have used these materials in photoelastic tests to model the responses due to dynamic loading. Indeed, the discussion of Section I and the theoretical method of Section II suggest this approach. It is therefore worthwhile making brief mention of the possibility of using photoviscoelastic techniques for assembling experimental data against which to compare the approximate analytical method proposed, although it is likewise appropriate to state those problems which are believed to most impede progress in using birefringent methods to experimentally analyze wave propagation in solids.

First, the quantitative determination of the stresses at an internal point requires in general three separate pieces of information, notwithstanding the important information which can be deduced solely from isochromatic patterns especially at free or normally loaded surfaces or along lines of symmetry. These are usually the isochromatics, isoclinics, and isopachics. In the dynamic situation, it appears impractical to observe all three quantities simultaneously at a given time, and hence the test must be repeated several times - once for the isochromatics, once for each isoclinic (say, every ten degrees between zero and ninety), and once for isopachics. Thus one must assume identity of loading and material response in those five to ten tests in order that the individual data be comparable, or be content with interpolated data rather than direct measurements. This procedure is not necessarily inaccurate but must be carefully considered in reporting final results.

Second, it has been found that another point of considerable confusion usually arising during the analysis of the isochromatics is the interpretation of the fringe patterns made from essentially white light sources. The unfiltered light prevents accurate resolution beyond four or five fringes, due to interference of the various wave lengths. This common static effect has been observed dynamically by Arens at GALCIT and by Cole, et al(17) at the Naval Medical Field Research Laboratory. Depending upon the light source, exposure time and filters used, this effect may or may not be an important quantitative analysis item in a particular problem.

Third, the dynamic strain-optic law, or variation of material properties and optical response with rate of loading, must be known for the material under investigation. For CR-39, Clark (18) has presented some results which indicate the type of variation to be expected, while Durelli (19) has given some results for Hysol 8705, a softer material. It is imperative that the material property variation and dynamic birefringence be obtained for the entire range of interest of the reduced strain rate, Ra_T.

Fourth, it is generally assumed that the isochromatic patterns will give wave front position with time. In the GALCIT work for example where a Hysol specimen has been loaded using a shock tube such that the wave travels over the surface faster than through the medium (Figures 10, 11), it would appear that the wave front is defined with reasonable accuracy by the leading fringe shown in the figures. It is necessary to point out that this is not the dilatation wave front, or even the isochromatic wave front (which in many cases does correspond to the dilatation front), but is the one-half order fringe front thus implying a larger angle for the zero or first signal front. That the effective speed of sound in the material is slightly higher may or may not be of first order importance in quantitative analysis.

With due regard to such difficulties as enumerated above, one should not conclude, however, that dynamic photoelastic analysis is impractical or too cumbersome, particularly if appropriate theoretical work accompanies the experiments. One important example is that theoretical predictions could be checked by computing the principal strain or principal stress difference from the theory and checking against the isochromatics from the experiment. While this procedure imposes more of a demand upon the theoretician-if the problem under consideration can be solved analytically - it may not be at all unrealistic under the circumstances.

With due regard for the above problem areas, the one which appears to cause the most difficulty is that concerning the material characterization. One of the first to discuss mathematical photovisco-elasticity was Mindlin (20). Recently Stein, Onogi and Keedy (21) have contributed additional theoretical work which is particularly interesting inasmuch as the chemical nature of the material is emphasized. In addition to the experimental work of Clark and Durelli previously mentioned, Theocaris and Mylonas (22) have published the results of their measurements for strain and stress optical coefficients in Hysol viscoelastic rubber. This latter work provides some concrete evidence that the two are not the same, although the range of strain rate is quite restricted.

These tests point up the controversy that has existed for some time as to whether and when the fringe order of given photoviscoelastic materials is strain and/or stress rate sensitive. The question usually takes the form of postulating a stress optic or strain optic coefficient, C_σ or C_ϵ respectively, relating principal stress or strain difference with fringe order, n, viz,

$$\eta = C_\sigma\,(\sigma_1 - \sigma_2) \tag{33a}$$

or

$$\eta = C_\epsilon\,(\epsilon_1 - \epsilon_2) \tag{33b}$$

Depending upon the material, one law or the other, or a combination of both Eqs. (33a) and (33b) seems to fit the experimental data. For the case of linearly viscoelastic materials, however, it is believed this matter can be resolved. Furthermore, the work of Schapery(2) provides a thermodynamic basis for expecting such a behavior of the birefringence, as well as its suggesting the same type of dependence on reduced strain rate Ra_T, as found for mechanical properties (Figure 1).

Suppose now the time dependent shear response is characterized by

$$P'\,(\sigma_1 - \sigma_2) = Q'\,(\epsilon_1 - \epsilon_2) \tag{34}$$

where the (reduced) time dependent linear operators P' and Q' are, for example (analogous to the treatment in Section I)

$$P' = \sum a\,(\partial^n/\partial t')\quad;\quad Q' = \sum b_m(\partial^m/\partial t'^m) \tag{35}$$

and the reduced time, t', is related to the physical time, t, through (7)

$$t' = \int_0^t du / a_T \left[T(u) \right] \qquad ; \tag{36}$$

the $a_T(T)$ is the temperature dependent shift factor (12). Then the Laplace transforms of Eqs. (33) and (34) with respect to reduced time are related. Specifically, one has

$$\bar{n} = C_\sigma (p) \left[(\bar{\sigma}_1 - \bar{\sigma}_2) \right] = C_\sigma (p) \left[Q'(p)/P'(p) \cdot (\bar{\epsilon}_1 - \bar{\epsilon}_2) \right] \tag{37}$$

so that upon defining the operational shear modulus as $G(p) = Q'/P'$, or its inverse shear compliance $D(p) = P'/Q'$, one has

$$C_\epsilon (p) = C_\sigma (p) G (p) \tag{38a}$$

and of course alternatively

$$C_\sigma (p) = C_\epsilon (p) D(p) \tag{38b}$$

thus illustrating that the stress and strain optical coefficients cannot in general be independent, but are necessarily related through the (time-temperature dependent) modulus of the viscoelastic material. The predicted dependence upon reduced time, or strain rate, permits a convenient extension of the physically realizable strain rates in the laboratory to a large range of reduced strain rates by testing at various (constant) temperatures. Indeed, preliminary GALCIT results from such tests for an equivalent log $(a_T \omega)$ range of nine decades correlates qualitatively on the basis of Eq. (38a). It would appear that the laboratory results presented by Theocaris and Mylonas - which are precisely of the type needed to examine the postulated dependence Eq. (38) - could also be extended by this technique.

CONCLUSION

Some interesting conclusions emerge. The one-dimensional problem indicates the behavior to be expected from waves traveling in realistic viscoelastic materials. The high frequency components of the response are very rapidly attenuated with distance and the major portion of the stress or displacement rise occurs near the "rubbery" arrival time. Furthermore the methods of inversion proposed here can be made as accurate as desired for any application simply by taking more terms in the series with no essential increase in mathematical difficulty. It is felt that this is a distinct advantage over the analytical model representation used in previous treatments of wave propagation in viscoelastic media. The method also appears to be equally applicable

to two-dimensional problems and extensions to such situations are currently in progress at GALCIT.

On the experimental side the use of alternate instrumentation such as electrical and mechanical strain gages should not be disregarded, but it is suggested that careful consideration be given to the use of photoviscoelasticity as a practical means of analyzing stress fields, particularly in complex shapes. Its simplicity is attractive (Figure 11) although its quantitative success will depend significantly upon the careful consideration of properly determining the mechanical properties and dynamic birefringence of the model material, and developing satisfactory analysis techniques for simultaneous determination of individual stresses at a point.

It is felt that a judicious combination of the approximate analytical technique and photoviscoelastic analysis will permit, even now, a more effective approach to this general class of problems which Hunter (1) implies has for the most part resisted both analytical and experimental attack.

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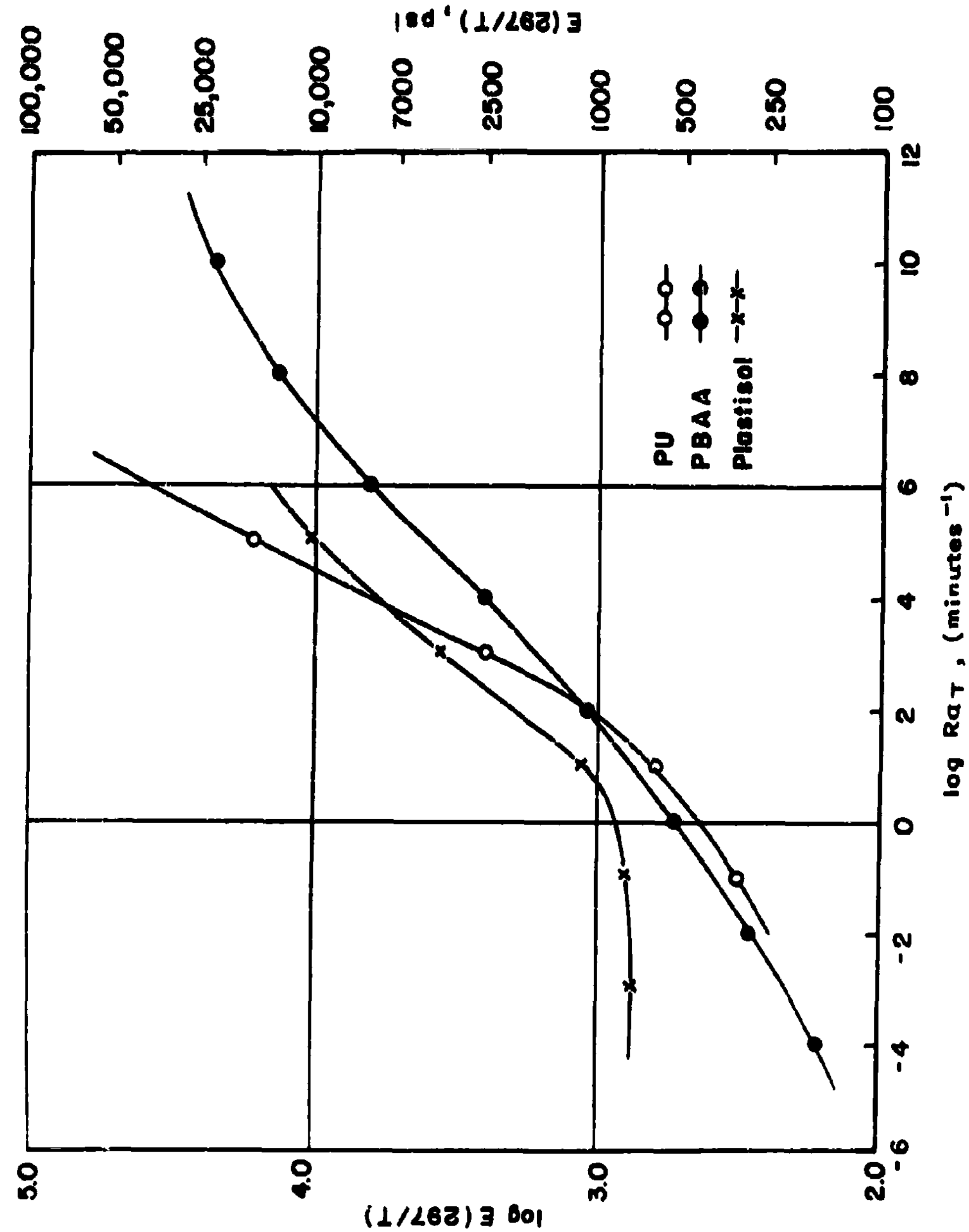


FIGURE 1a - NORMALIZED MODULUS VARIATION WITH REDUCED STRAIN RATE

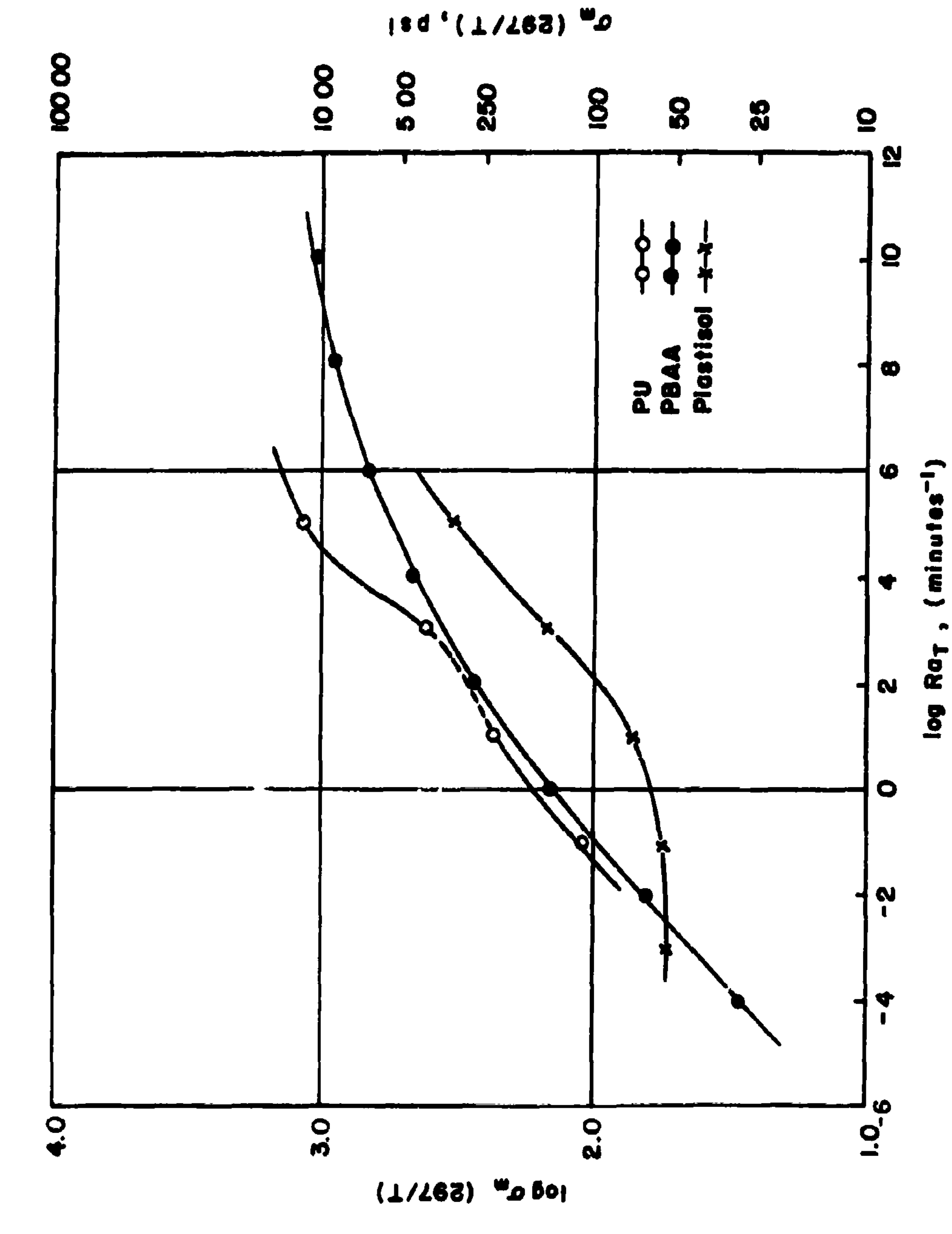


FIGURE 1b - NORMALIZED MAXIMUM TENSILE STRENGTH VARIATION WITH REDUCED STRAIN RATE

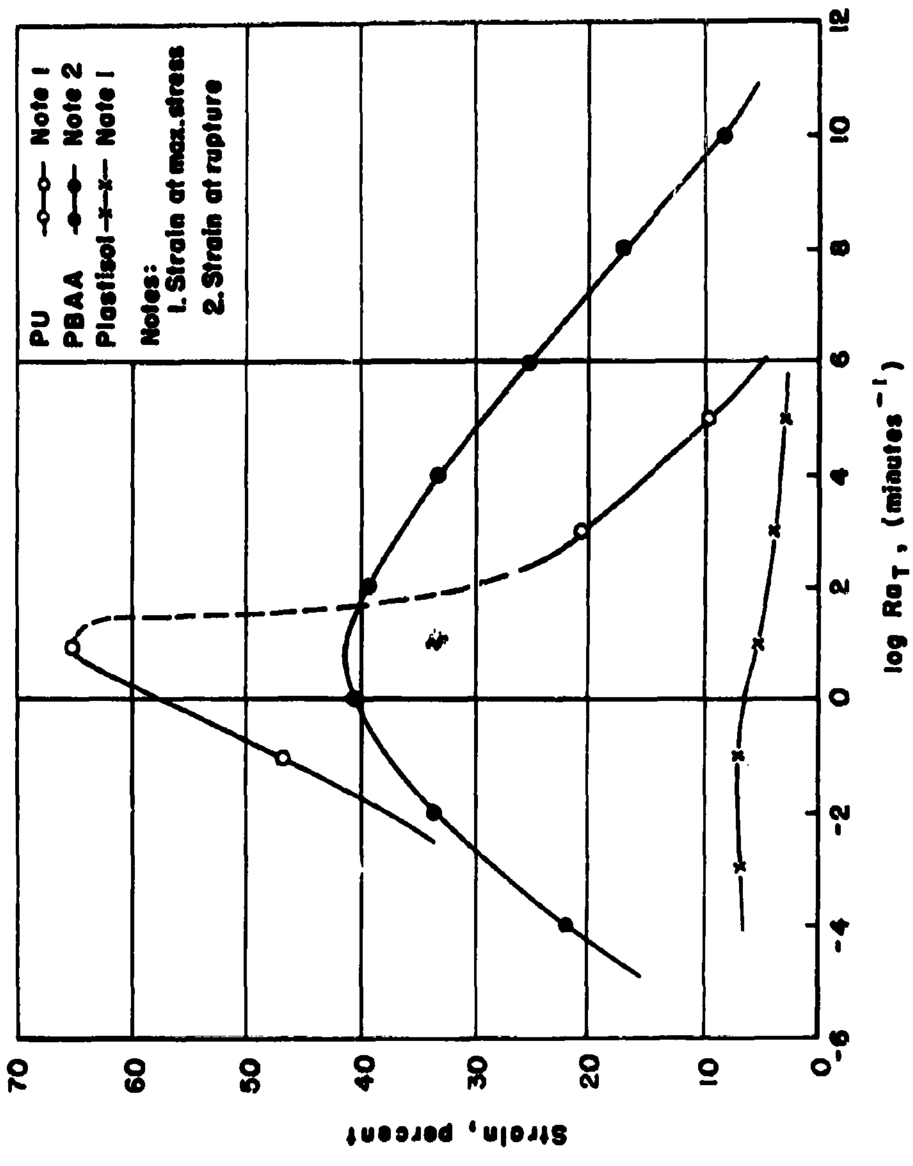


FIGURE 1c - CRITICAL STRAIN VARIATION WITH REDUCED STRAIN RATE

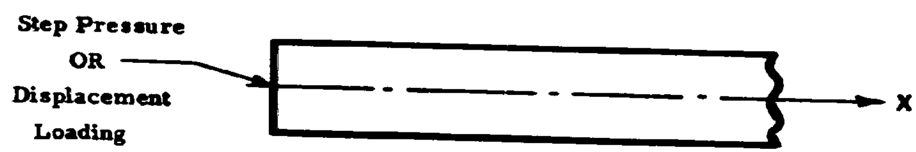


FIGURE 2 - GEOMETRY OF VISCOELASTIC ROD

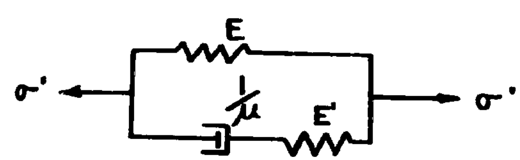


FIGURE 3 - STANDARD LINEAR SOLID MODEL

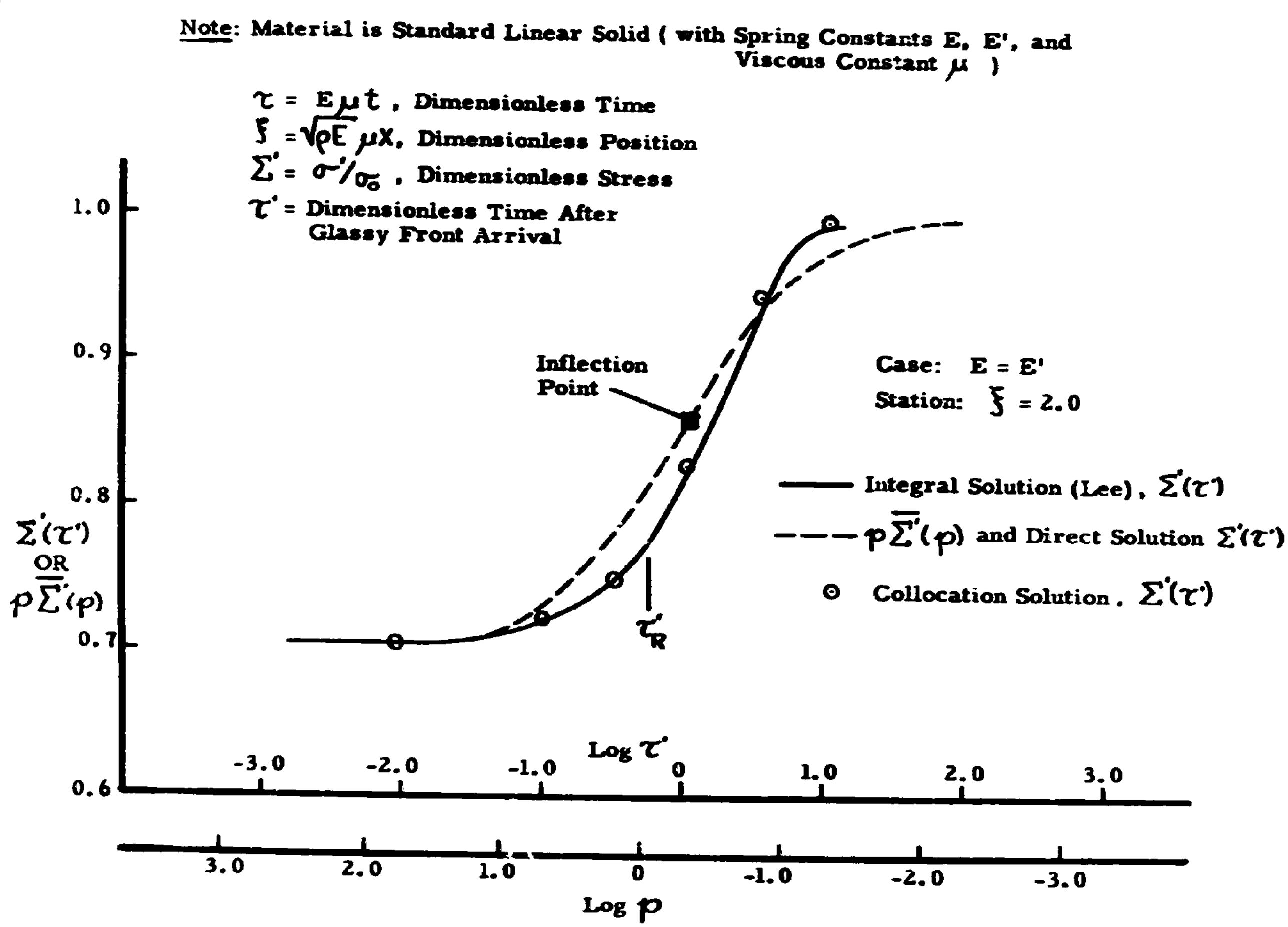


FIGURE 4 - WAVE PROPAGATION IN VISCOELASTIC ROD OF STANDARD LINEAR SOLID MATERIAL FOR STEP STRESS INPUT

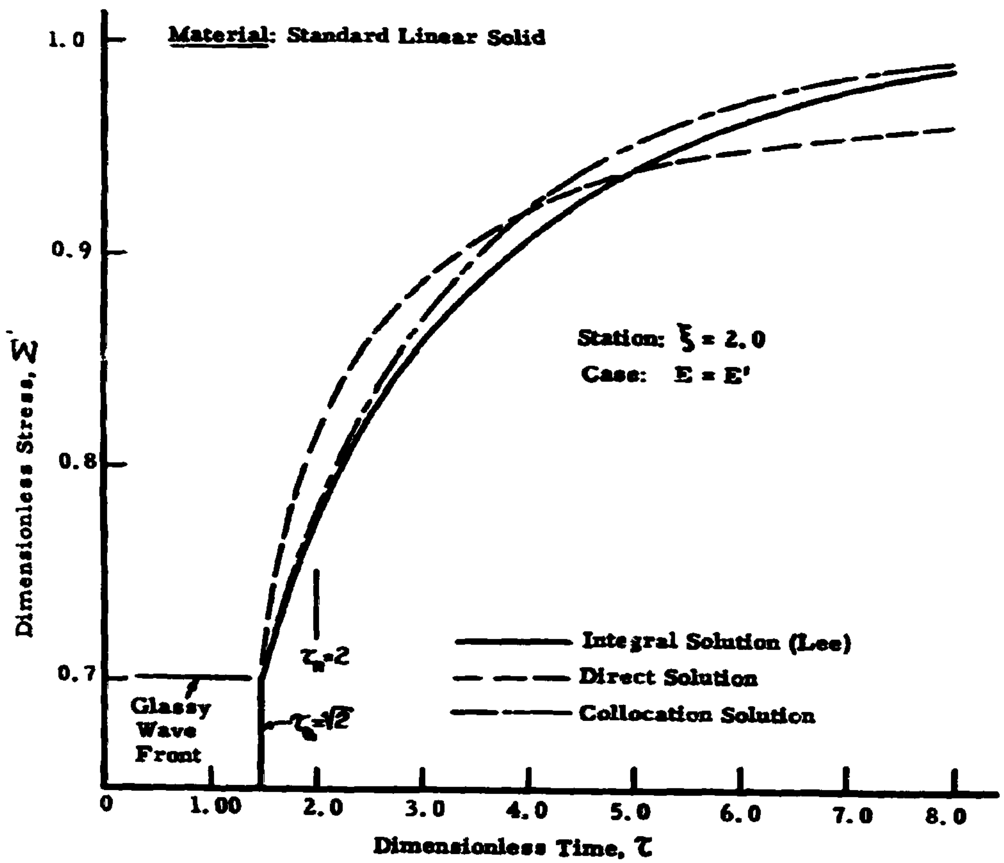


FIGURE 5 - COMPARISON OF SOLUTIONS FOR WAVE PROPAGATION IN VISCO-ELASTIC ROD FOR STEP STRESS INPUT

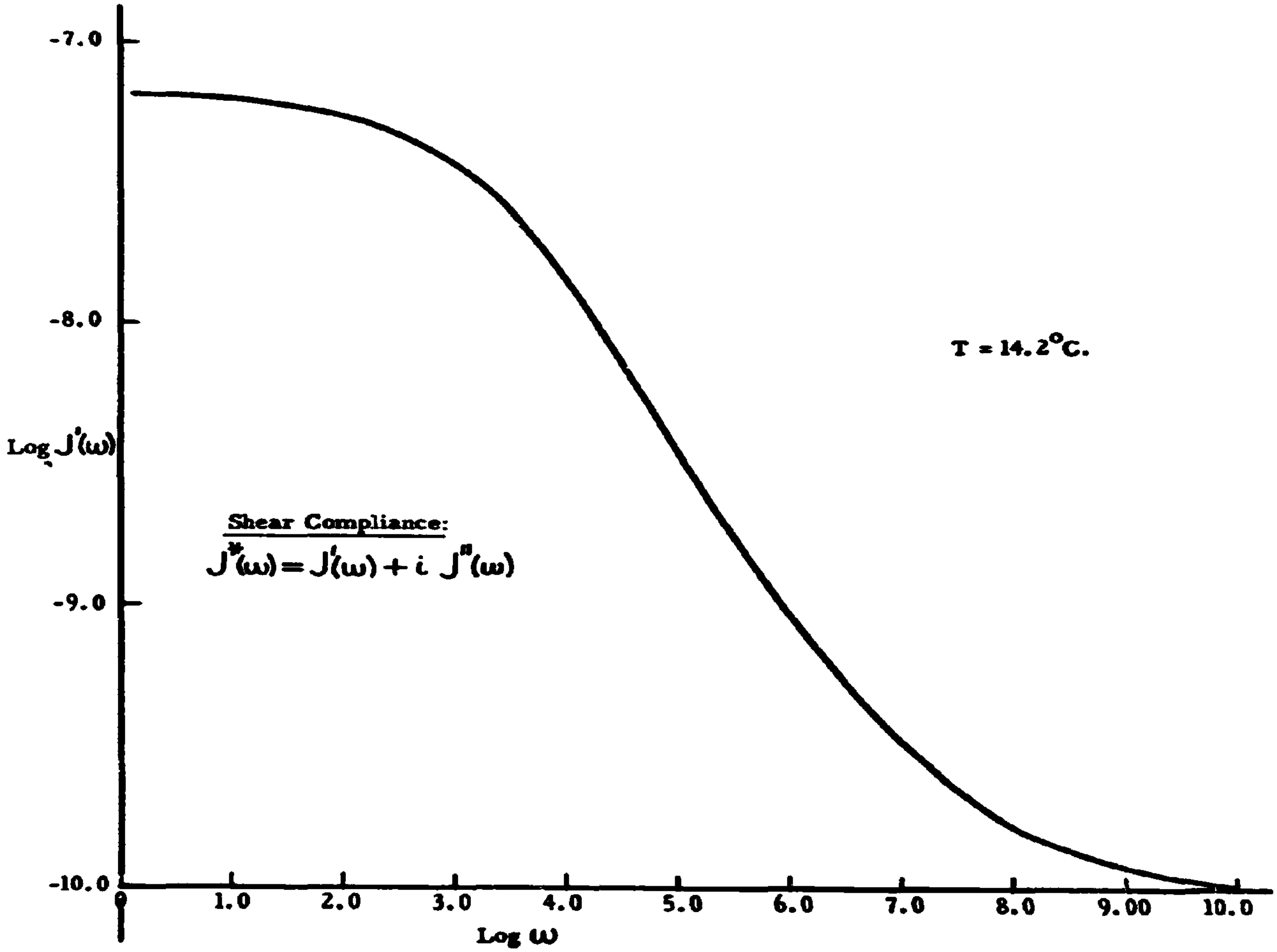


FIGURE 6 - REDUCED SHEAR COMPLIANCE (REAL PART) OF POLYURETHANE MATERIAL

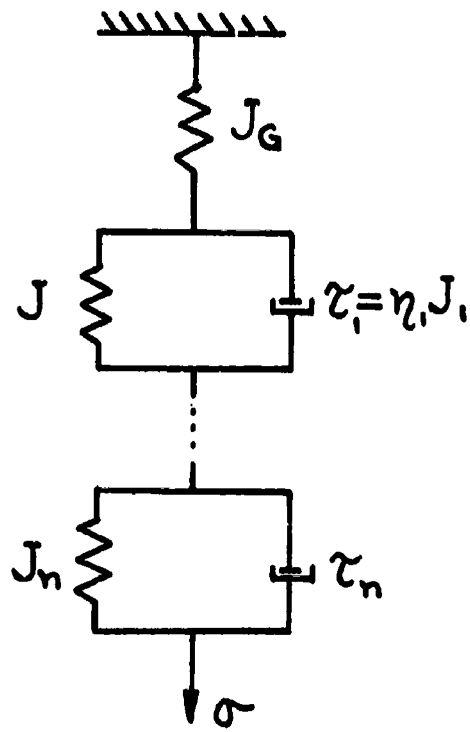


FIGURE 7 - REPRESENTATION OF REALISTIC MATERIAL COMPLIANCE BY KELVIN MODEL

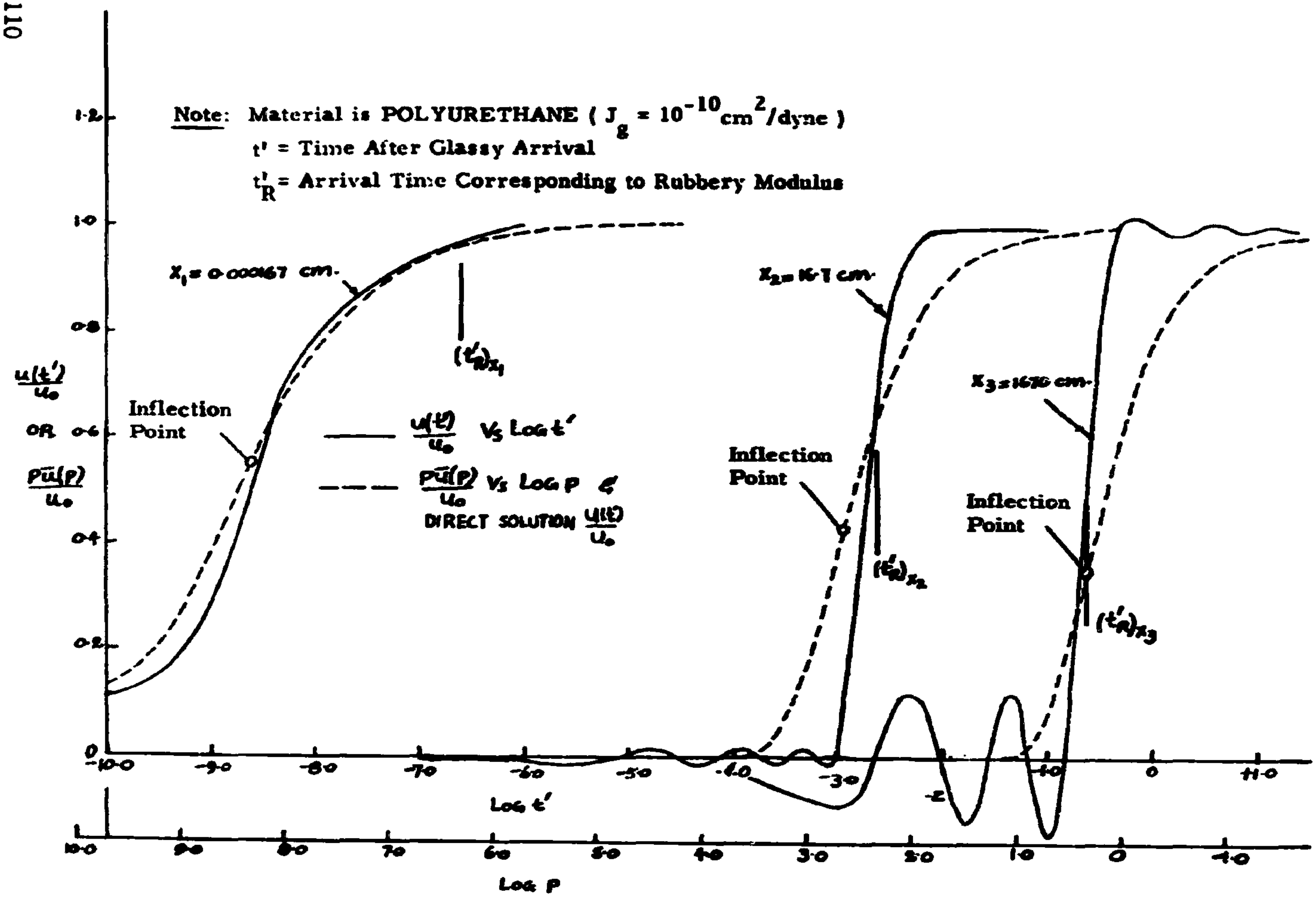


FIGURE 8 - WAVE PROPAGATION IN VISCOELASTIC ROD (RESPONSE TO STEP DISPLACEMENT INPUT)

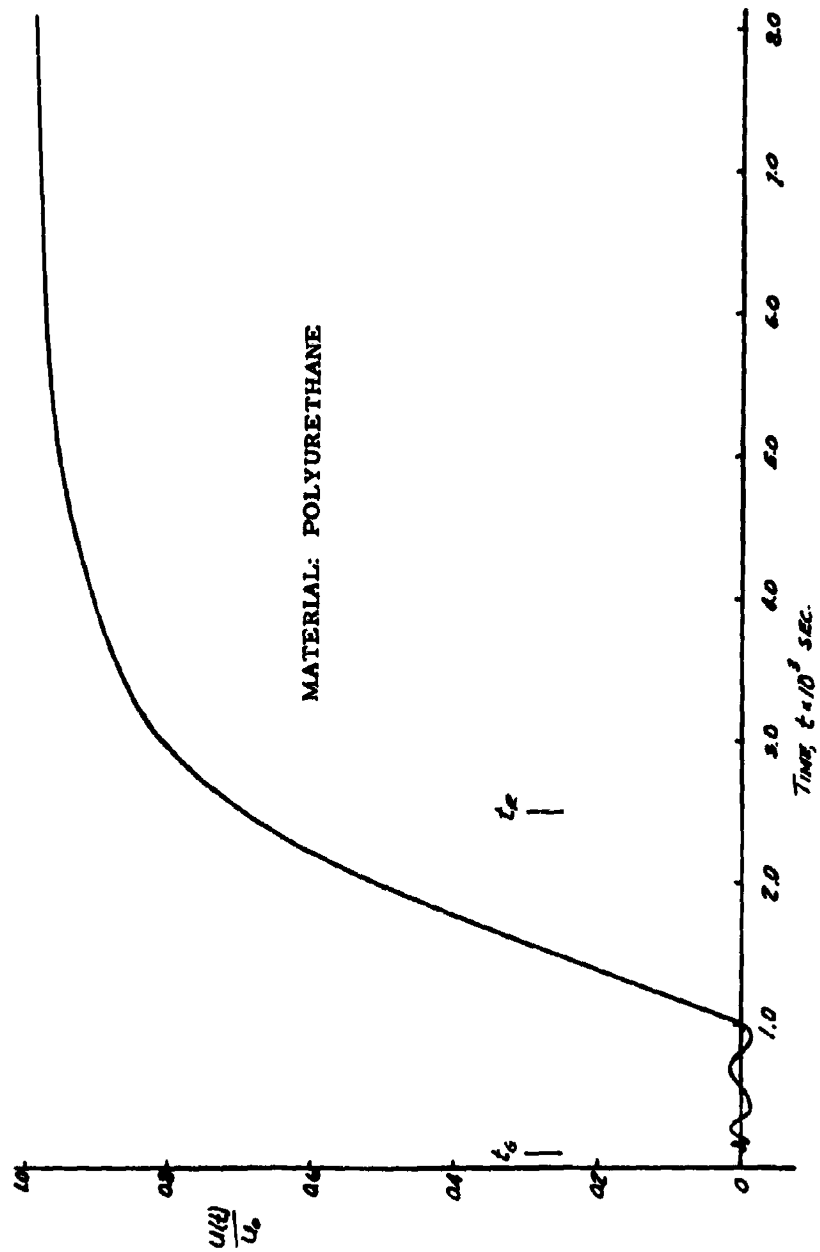


FIGURE 9 - WAVE PROPAGATION IN VISCOELASTIC ROD FOR STEP DISPLACEMENT INPUT; x_2 = 16.7 cm.

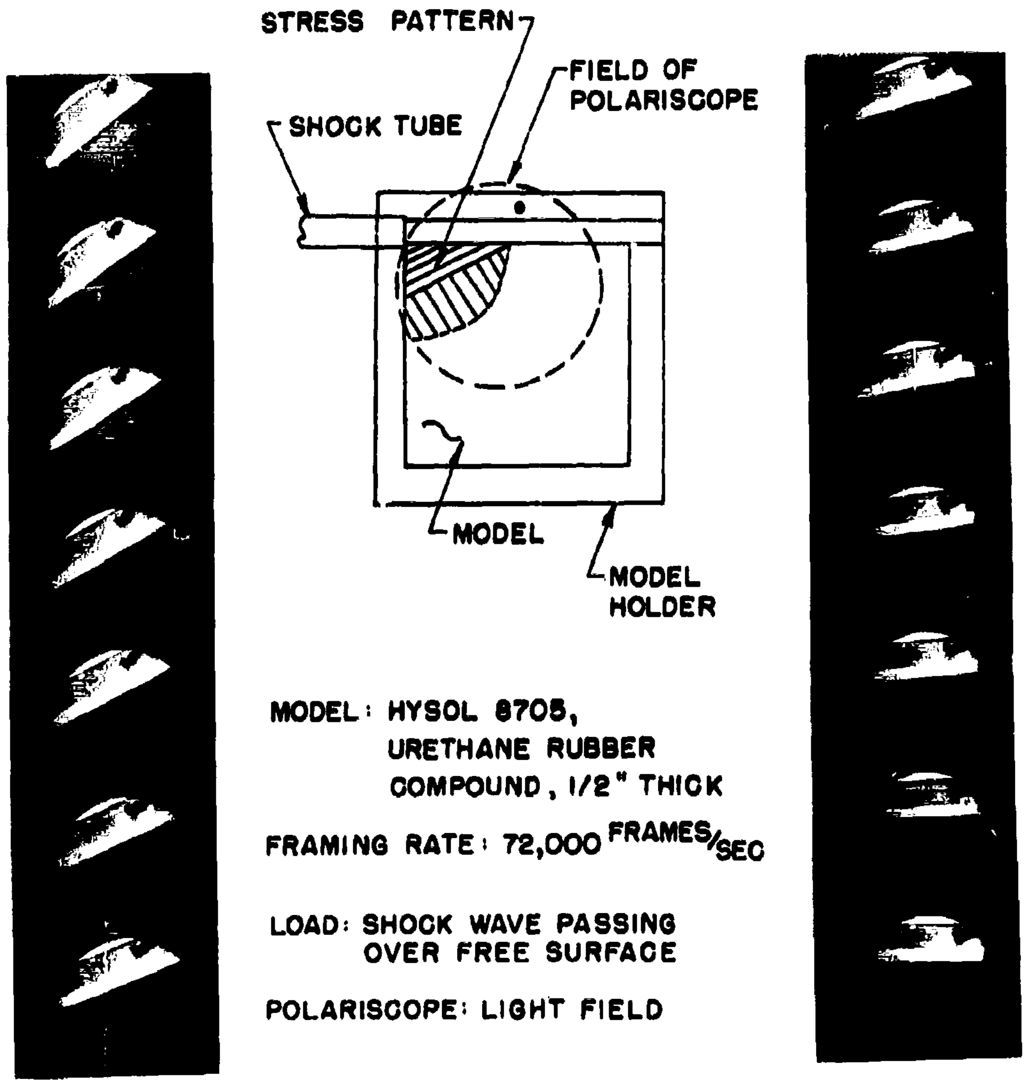
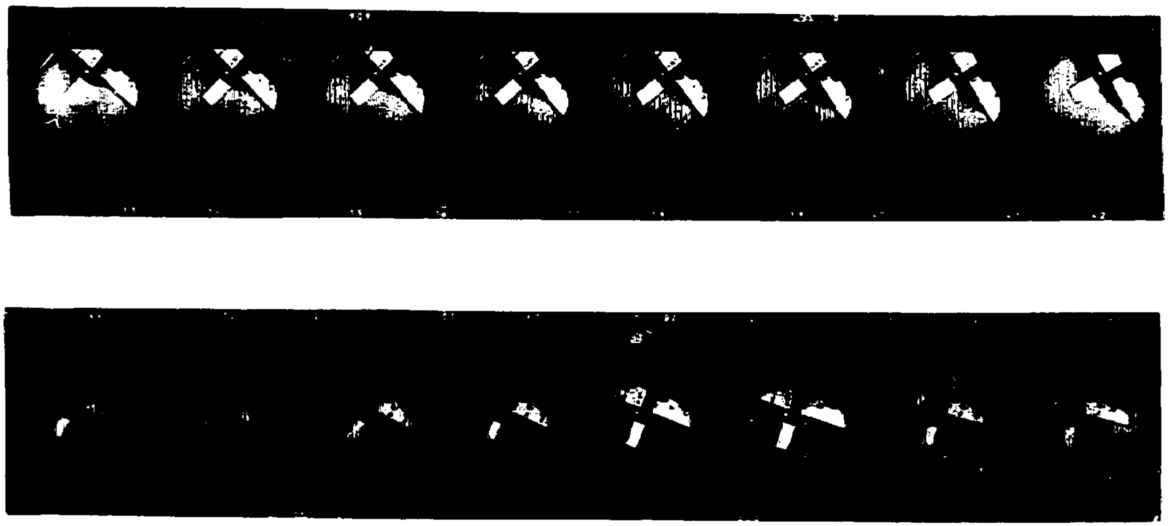


FIG. 10- DYNAMIC STRESS DISTRIBUTION FROM SHOCK WAVE PASSING OVER LOW MODULUS PHOTO-ELASTIC MODEL



MATERIAL: HYSOL 8705

FRAMING RATE: 89, 310 FRAMES/SEC

SHOCK SPEED: 2300 FT/SEC

TIME MEASURED FROM START OF SHOCK WAVE PASSAGE OVER SPECIMEN

NOTE: CUT-OUT $2\frac{1}{2}$" x 1" WITH $\frac{1}{8}$" COVER

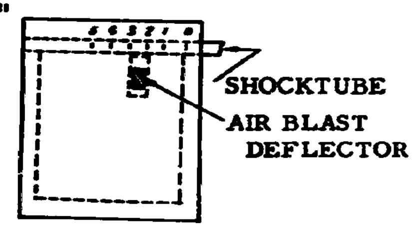


FIGURE 11 - DYNAMIC STRESS DISTRIBUTION AROUND A COVERED DEEP CUT-OUT DUE TO SURFACE SHOCK WAVE LOADING

DISCUSSION

DR. DRUCKER

Thank you very much. We have a little time for discussion, if someone has some comments, or questions you would like to ask.

DR. HOPPMAN

I'd just like to make a comment on this approach to the problem in general. I've thought about this for a long time and have some opinions about it and the Brother's work is very interesting, but I would like to draw attention to the contrast between the speculative writings of Brother Arenz and Doctor Ericksen. Now, his is speculative also, and he makes no pretense--correct me if I'm wrong. There is no pretense about immediate correlation between the physical and the mathematical speculative. I think the importance of Ericksen's work is that he provides scope. Now, he sticks his neck out doing this and I think he's quite well aware that he sticks his neck out with this approach to the problem, but it allows an opportunity in the long run to fit into the scheme of analysis what we will ultimately find in the physical realm. The criticism I have to make of your approach, Brother Arenz, is that it is an exercise in division of Laplace transforms. Ledderman started this business you are dealing with now quite a few years ago at the Bureau of Standards, dash pots and springs. I think, at least, you should imply in the beginning that it is limited to springs and bars because if you begin to apply the analysis to three dimensional bodies like spheres and so on, first you encounter considerable difficulties in the analysis, and secondly, you have no hope that this is going to conform to real material. You imply a glassy state of matter and so on. But in the long run, you would have to go to the laboratory to experiment with this to find out if it does or doesn't. You can't pontificate about what is going to be. This is easy enough to say; everybody uses this as a dodge, but I am quite sincere in my statement that ultimately we have to do it, so if you'll permit the contrast in Dr. Ericksen's approach, you will have the flexibility when you ultimately come to the final problem. He has been proven wrong many times, too, but it leaves the scope of the adjustments to the final physical reality. I hope I make the point clear.

BROTHER ARENZ

Yes. It's a good point. As I mentioned, I think that it is certainly advisable to go to experimental techniques to carry this further to verify it. I think that the advantage of the inversion technique indicated here, which I didn't intend to stress too much, is simply that it has the feature that it can be made more accurate with no essential increase in mathematical difficulty, and this has been indicated to me in some of the work. The other thing is that it is possible to use it in more than one-dimensional problems. In two-dimensional problems the amount of computation does increase, but with no essential increase in difficulty; these techniques are mainly an inversion of matrices which high speed computers can handle nicely. It's a simple operation for them. That is a fundamental feature of the technique; you must invert a matrix to represent the series. Well, it has been extended to the two-dimensional problems and I don't know about the three-dimensional case, but there are some results to the two-dimensional problems.

DR. DRUCKER

I'm sorry we can't have any more questions now. We stand adjourned until 1:30.

TECHNICAL SESSION II

WAVE PROPAGATION PHENOMENA AND STRUCTURAL RESPONSE

Professor Werner Goldsmith, Ph.D.
Session Chairman

University of California

TECHNICAL SESSION II

INTRODUCTORY REMARKS

COLONEL L. R. STANDIFER

We are moving into our second technical session titled the "Wave Propagation Phenomena and Structural Response Session." We have as Chairman, Dr. Werner Goldsmith from the University of California, Berkeley. Dr. Goldsmith took his B.S. and M.S. degrees in Chemical Engineering in 1944 and 1945, respectively, from the University of Texas, and his Ph.D. degree from the University of California, Berkeley, in 1949. He has approximately 40 plus technical publications to his credit in the field of propagation dynamics and experimental stress analysis. Doc has a book "Impact" published in 1960. He has a background both in industry and in the education world, recently being with Westinghouse Electric Corporation and moving quite rapidly through the ranks to Professor at the University of California. He was a Guggenheim Fellow in 1953 and 1954 and at the present time he is a faculty investigator on the various problems involving impact wave propagation and penetration. At this time it gives me great pleasure to introduce the Second Session Chairman, Dr. Goldsmith.

DR. WERNER GOLDSMITH

Chairman, Session II

Thank you, Colonel Standifer. Lady and gentlemen, before opening the technical discussion this afternoon I would like to make a few comments concerning the general field of wave propagation.

Waves of appreciable amplitude can be produced in a solid body or structure either as a result of collision with another solid resulting from an impingement by a tidal wave, blast, or other fluid mechanism, or by means of the release of an internal nucleus of stress such as the case of an earthquake. Studies concerning wave propagation have been pursued for many years; in fact, more than a hundred years. But even today, in spite of considerable advances in both the mathematical procedures and techniques, as well as the experimental observations of these phenomena, our knowledge of the mechanism of the progression of such pulses and their effects on the transmitting medium is somewhat limited. Rigorous analysis of such processes cannot be performed at the present time unless restricted either to a class of bodies with very idealized properties, or with respect to some very simple geometry. One of the two major difficulties which are encountered in the theoretical approach in the solution of wave propagation phenomena concerns the proper, yet sufficiently simple description of material behavior. Almost universally, in the area of applied mechanics at any rate, such behavior is represented on a microscopic basis; in other words, a continuum or presentation of the action of the material. For many metals the stipulation of the homogeneous isotropic elastic solid has proved to be adequate under certain respective circumstances, provided that the stress amplitude considered did not exceed the elastic limit of the material. For others, such as for example lead, even very low amplitude pulses cannot properly be represented by an elastic description. Consequently, we are in search of relatively simple equations of state or constitutive equations with which we can predict the behavior of the material with sufficient accuracy to allow a

relatively simple, yet adequate, description of the process. At somewhat higher pressures, metals of course are subject to permanent deformation. A complete knowledge of the interrelation of the pertinent variables in the plastic range has not been completely developed, although considerable progress has been made in the last decade.

As an example of this I would like to cite some of the very well known work in the area of description of perfectly plastic solids or elasto-plastic solids which has found wide favor among many investigators. At extreme pressures approaching the magnitude of the modulus of the elasticity of the material, the ordinary solid is usually treated as a fluid since compressibility effects which are neglected in the elastic range must be definitely considered in this treatment of the problem.

There exists of course a transition region which has not been very well explored up to date, but the mechanisms for exploration both theoretically and experimentally are presently within our hands and it is hoped that more information will be gained in this transition region.

I need not dwell upon the approximations that are made in the analysis of the modulus presentation of the material. Suffice it to say that many different characteristics actually affect the behavior of the material and these cannot always be represented on a continuum basis. For example, such effects as changes in the dislocation, crystal and transformation, shattering, pulverization, vaporization of the material and shock heating must be represented in a complex thermodynamic mechanical equation of state.

Fortunately, as we have learned earlier today, it is apparently possible to use the researches of the metallurgists in their presentation of materials from dislocation theories to predict the applicability of one or more equations of state. This is an area which shows great promise, which we will hear more about in the remaining sessions.

Finally, I would like to say one additional word with respect to wave propagation phenomena. It is long known that the distribution of energy through a structure will enable the structure to either survive or fail under the action of what is called here a hostile environment. The determination of the response of such structures to impulsive loading both at the input position and at all vital stations throughout the complex is of extreme importance here. Failure of a system may have many criteria. Intense deformation, for example, may be permitted at one or more stations, yet very small deformations would lead to failure of the system at others. Consequently, it is not always possible to say that a structure can or cannot perform under a given circumstance. Material properties of such structures are not always well known and certainly the loads are not, and the analysis of such systems requires not only a considerable amount of ingenuity but also a fairly good background in mathematical techniques.

We are extremely fortunate this afternoon to have a panel of very highly expert speakers who will inform us more about various phases of wave propagation, material properties, dynamic response. The first speaker this afternoon is Dr. John Percy. He comes to this country via New Zealand and Cambridge where he received his Ph.D. degree. He has spent approximately four years lecturing at the University of California, Oakland, and is now associated with the Aeroelastic Structures Research Lab at MIT. He will speak to us on the subject of Wave Propagation in Uniaxial Strain. Dr. Percy.

DR. PERCY

Madam, gentlemen. As Doctor Goldsmith has just said, if we are to analyze the observed effects of high impulse loading on structures, and if we wish to predict such effects, we must have a very good understanding of the behavior of the material under the extreme conditions which are generated; and it is this, I think, which motivates our interest in wave propagation in uniaxial strain. For if we wish to find out what the material properties are, we should choose for this investigation a simple geometry.

WAVE PROPAGATION IN UNIAXIAL STRAIN

by

John H. Percy, Ph.D.

Massachusetts Institute of Technology

WAVE PROPAGATION IN UNIAXIAL STRAIN

John H. Percy

Massachusetts Institute of Technology

ABSTRACT

Interest in this topic centers on the constitutive equation or behavior of materials under the extreme conditions of high impulse loading. Recent and current work in the field is treated generally and one area, the elastic-plastic behavior of aluminum at comparatively low pressures, is considered in detail. The outlook for future work is discussed.

WAVE PROPAGATION IN UNIAXIAL STRAIN

INTRODUCTION

If two of the principal components of strain are zero, the strain is uniaxial. This condition arises in longitudinal plane motion where if u, v, w are the x, y and z components of the displacement vector, we can write $u = u(x)$, $v = w = 0$. Longitudinal plane waves subject the material to uniaxial strain. Such wave motion may be generated in an infinite plate where the plane motion of one face is propagated through the thickness of the plate. This paper deals only with such a configuration.

Recent and current work on wave propagation in uniaxial strain is surveyed; the aim is to present a picture of the state-of-the-art. The survey is representative, not exhaustive. One particular area which is currently being studied at M.I.T., namely, the elastic-plastic behavior of aluminum at comparatively low pressures, is dealt with in some detail. In this section some of the problems touched on in the general survey can be looked at more closely. The paper ends with a brief discussion of the outlook for future work.

SECTION I: GENERAL CONSIDERATIONS

In order to analyze the observed effects of high impulse loading on structures and to predict such effects, the behavior of materials under extreme conditions must be well understood. The work to be described in this paper is directed toward such an understanding.

It is best to use a simple geometry to explore material properties since the effects of material behavior can then be readily isolated from effects arising from the mechanics of wave propagation and the specimen geometry. Two suitable simple configurations are uniaxial stress and uniaxial strain. Both have advantages and disadvantages.

Uniaxial stress (wire and rod) experiments reveal dynamic

material behavior in the absence of the high hydrostatic pressures which are inevitable in uniaxial strain. However, they suffer from the serious disadvantage that the stress is not in fact uniaxial. Poisson effects induce radial motion and hence radial stresses. Without careful analysis of the complex three-dimensional wave system, which the experiment was originally designed to avoid, only the gross features of the behavior, which are negligibly affected by the lack of ideal axiality, can be interpreted. For uniaxial strain (plate) experiments on the other hand the axiality assumption is closely realized. The inadequacies of these experiments arise rather from difficulties of instrumentation.

An experiment in longitudinal plane wave propagation is conducted as follows. The specimen is a plate and a disturbance is generated uniformly over one surface which sets it in plane motion. The transient response is observed as well as can be, usually on the other surface. All observations are made at times before disturbances from the edge of the plate can reach the instrumentation. The observations are therefore identical with those that would be made on a plate of infinite extent.

Ways in which the plane disturbance can be produced are illustrated in Figure 1. Method (i) offers the greatest control over the conditions of the experiment but if the target is large enough for proper instrumentation the projectile is also large and the maximum velocity achievable is correspondingly small. This method can be used to induce pressures up to about 100 kilobars. In contrast, method (iii) can be used to induce pressures of the order of megabars. The highest reported pressure generated by plate impact is 10 megabars (Ref.1) but the way in which this was achieved is not described.

Measurements of the response are limited by the short times available for observations and by the time resolution required; wave speeds are of the order of 0.5 cm per microsecond. Instrumentation of the impact face and of points within the target is obviously limited. Observations on the rear surface are the principal source of information on behavior. There are a number of optical and electrical methods used which have their particular ranges of applicability. The results of an experiment will usually give some or all of: the velocity of the driver plate, the transit times of disturbances, information about the rear surface motion.

From this there is little that can be immediately backdeduced about material behavior. The procedure must be, rather, to compare the observed behavior with that predicted by the mechanics of wave propagation for a supposed consti-

tutive equation. Parameters in the constitutive equation can be evaluated when the prediction agrees qualitatively with observation.

SECTION II: A SURVEY OF RECENT AND CURRENT WORK

There are a number of different aspects to work in the general field covered by this paper. The first and most important of these is the determination of the Hugoniot relationships for various materials when they are loaded to arbitrarily high pressures. Here it is the initial impulsive loading of the material which is being studied and for most materials this is a shock loading since their compressibility decreases with increasing pressure. Conservation equations for uniaxial flow may be used to analyze the experimental data and express the loading behavior of the material as a Hugoniot curve, or locus of states which can be reached in a shock loading, from zero pressure. The most extensive program has been conducted at the Los Alamos Scientific Laboratory, where the loading Hugoniots of many metals have been measured up to pressures of about two megabars (Refs. 2,3). In the Sovient Union parallel work has been done by Al'tshuler and his collaborators (Refs.1,4,5). Non-metallic materials have also been studied. A summary of work on rocks has been given by Lombard (Ref.6). Quartz has been studied both by Wackerle (Ref.7) and by Fowles (Ref.8). A recent Soviet paper gave the Hugoniot curves for liquid nitrogen and solid carbon dioxide up to about 0.5 mb (Ref.9).

Except for an elastic forerunner at low pressures, a single shock wave is usually observed. In most cases, both metals and rocks, it is found that for varying shock strengths the relationship between the shock velocity and the material velocity behind the shock is linear. The assumption of this empirical result considerably facilitates the derivation of constitutive relations from the results of the shock-compression measurements. Not all materials show a single wave structure. Those materials, notably iron, in which the shock compression induces a phase transformation show a compression wave with a step structure, the shock wave up to the transition pressure outpacing the shock wave which further compresses the material. The shock-induced phase transformation in iron and

steel has been particularly studied (Refs. 10, 11, 12).

The statement made above that a single shock is usually observed is true only for strong shocks. For lower maximum pressures (e.g. about 105 kb for 6061-T6 aluminum), the compression is by two shock waves. The first of these is identified as an elastic wave and compresses the material to the Hugoniot elastic limit (about 6.5 kb for 6061-T6 aluminum). The following plastic wave involves slip and a relaxation of the stress anisotropy. Others have remarked before (Refs. 3, 13) that, because of the well-defined and simple geometry of the uniaxial strain configuration, experiments in elastic-plastic wave propagation are particularly suitable for a study of the yield phenomenon. In view of this it is surprising that elastic-plastic behavior has not attracted more attention. There have been theoretical analyses by Wood (Ref. 14) and Morland (Ref. 15). Experimental work has been reported for iron (Ref. 16) and for aluminum (Ref.17). The elastic-plastic behavior of aluminum will be discussed in detail in Section III.

Also associated with stress waves in uniaxial strain is the study of spallation. It is important for engineering reasons, and also promises to reveal much about the fracture process which static experiments conceal. Since spallation is to be treated elsewhere in the symposium, it will not be discussed here.

Apart from spallation studies the release or tensile behavior of materials shock-loaded in compression has received very little attention. Experiments conducted by Lundergan on aluminum have been analyzed by Herrmann, Jones and the author at M.I.T.; this work is dealt with in Section III. Much more intense loading, in this case of copper, has been reported by McQueen and Marsh (Ref. 18). It appears that the material reached an ultimate or yield strength in tension at 150 kb or more. Here the yield strength is to be interpreted as the stress beyond which a decrease in density is accompanied by a decrease in tensile stress. An interesting experiment by Al'tshuler and others (Ref. 5), not strictly a uniaxial strain experiment, measured the velocity of the front of the release wave propagating into material which had been shock-compressed to about 0.5 megabar. For copper and iron they found the velocity corresponded closely with the longitudinal sound speed to be expected at that pressure, whereas for water it corresponded with the bulk sound speed. The inference is that in spite of the success of the hydrodynamic theory in predicting effects in metals at these pressures, they do retain some

shear strength. These are all but first steps in the study of release waves and behavior in tension. Here is an important unexplored field.

So far covered are what might be called the primary topics in longitudinal wave propagation: loading behavior, unloading behavior and spallation. The object of the work is the observation of physical behavior. Associated theoretical work has attempted to describe the observed behavior in terms of physical processes and models, and to correlate it with behavior under other conditions. To support these investigations into the basic physical processes, experiments have been devised which look more closely at the details of the shock-wave propagation. The work on phase transformations in iron, cited above, is of this kind. So too is the observation of non-mechanical effects; for example, the piezoelectric and optical effects in quartz (Ref. 19), changes in electrical resistivity in insulating materials (Ref. 20). Post-mortem examination of the metallurgical changes which are a consequence of shock loading provide important evidence of the nature of the physical processes which have taken place (Refs. 21, 22, 23).

Perhaps the most important detail which attracts study is the nature of the shear yield process which is observed as the plastic or hydrodynamic wave in metals. There is a basic conflict between the assumption of a hydrodynamic shock wave, and the well-established model of dislocation and defect propagation for the yield process. Yielding is not instantaneous.

To resolve the conflict a strain-rate effect is proposed as a refinement to the mechanical description. The rate of plastic strain, and consequently the stress relaxation, is assumed to be governed by the difference between the instantaneous stresses and the stresses for static equilibrium at the same strain. This picture is plausible and instantaneous yielding is not, but there is little experimental evidence which can be used to make a quantitative estimation of the effect. After all, the Rankine-Hugoniot rate-independent theory does describe very well the observed results of shock loading. One cannot expect at the same time to have good evidence of rate dependence.

The relaxation process shows up most strongly in the profile of the shock wave, but a well-resolved observation of this profile is difficult indeed. For example, if we assume a limiting strain rate in aluminum of the order of 10^6 per second the duration of a 100 kb shock is of the order of 100 nanoseconds. The capability of resolving the profile is pos-

sibly just within the limits of present technology. Attempts which have been made (Ref. 24) in fact measure rear surface velocities; the instrumentation is being used to its limit and it is measuring an integrated effect of the passage of the wave through the specimen thickness. The results support the hypothesis of a rate effect in the yielding process but they do not enable a quantitative estimate of its nature to be made with any confidence.

Another way of approaching the strain-rate effect would be to propose a physical model for the process of yielding under shock conditions and to follow through its consequences quantitatively in terms of basic material properties. This cannot be done at present. The mechanism of shear flow in the shock is observed to be largely by twinning; a completely satisfactory model for the process of twinning does not exist. The best that can be done is to recognize the fact that yielding is largely a thermally-activated process and that the activation energy available is some function of the stress. A plausible strain-rate law equates the strain rate to a simple exponential function of the stress which is zero when the stress is in static equilibrium (Ref. 24).

SECTION III: THE ELASTIC-PLASTIC BEHAVIOR OF ALUMINUM

Recent work at M.I.T. (Ref.25) and work now in progress concerns the constitutive equation for aluminum at pressures up to about 10 kb. A discussion of this work follows, partly to illustrate in more detail some of the general remarks above, partly for its own sake.

If it is assumed that the wave structure in an impacted aluminum plate is an elastic shock preceding a plastic shock, the Rankine-Hugoniot relations may be used to deduce the Hugoniot centered at zero pressure from a series of impact experiments. Such a series of experiments on 6061-T6 aluminum alloy has been conducted by Lundergan at the Sandia Corporation (Ref.17). For impacts over a range of velocities, the projectile velocity, the elastic and plastic wave velocities and the rear surface velocity after the first (elastic) wave reflection are measured. The Hogoniot follows the longitudinal elastic response to the yield point and is thereafter displaced by a constant stress from the hydrodynamic Hugoniot. This is in

agreement with Fowles' results (Ref. 26) and the theory of Wood (Ref. 13).

Now in further experimental work Lundergan has used slant-wire resistors to follow the rear surface motion for about seven microseconds after impact (Ref. 27). The conditions of the particular shot which is analyzed here are shown in Figure 2. The observed rear surface motion is shown in Figure 3. This information cannot be used directly to back-deduce the material behavior. Rather, the expected rear surface motion for hypothetical constitutive equations is calculated and compared with the experimental result.

Calculations have been made with a finite difference machine program (Ref. 28) using the constitutive equation, based on the earlier results, shown in Figure 4. The results shown in Figure 3 agree closely with the observations. This is experimental evidence that the simple elastic-plastic theory of Wood (Ref. 13) is valid, under these conditions, for unloading behavior. At the same time the experimental information does not discriminate among a number of hypotheses for a more rational constitutive equation.

The most obvious criticism of this constitutive equation is that it is independent of the rate of strain. We have therefore calculated the rear surface motion with a strain-rate effect included. Following Malvern (Ref. 29) we postulated that at any stress beyond yield the material relaxes to the equilibrium stress at a plastic strain rate which is a function of the difference between these stresses. As a measure of the stress we took the von Mises effective stress which for axial symmetry is simply the difference between the axial and transverse stresses. The equilibrium stress at any strain was assumed to be the stress given by the constitutive equation of Figure 4. The plastic strain rate in the axial direction, $\dot{\epsilon}''$, was taken as follows.

If $\quad \sigma^* \geq Y \quad , \quad \dot{\epsilon}'' = a(\sigma^* - Y)\exp\left\{b(\sigma^* - Y)\right\}$

If $\quad Y \geq \sigma^* \geq -Y \quad , \quad \dot{\epsilon}'' = 0$

If $\quad -Y \geq \sigma^* \quad , \quad \dot{\epsilon}'' = a(\sigma^* + Y)\exp\left\{b(\sigma^* + Y)\right\}$

Y is the yield value of the von Mises effective stress, equal to 1.5 times the offset of the plastic curves from the hydrodynamic Hugoniot in Figure 4.

σ^* is the von Mises effective stress which is 1.5 times the difference between the axial stress and the stress given by the hydrodynamic Hugoniot at the same volumetric strain.

We took $a = 743\ kb^{-1}\ sec^{-1}$ and $b = 5.0\ kb^{-1}$

The result of this calculation is also shown in Figure 3. It certainly appears that the rate-dependent hypothesis is closer to the truth. Nevertheless the results of both calculations are within the experimental error. The form of the strain-rate function evidently may not be determined in this way.

Another point of interest in the constitutive equation is the change in the longitudinal elastic modulus with increasing pressure. This governs the velocity of the elastic release wave. In Figure 4 it is taken as constant. Finite strain elasticity theory enables us to get a better estimate of its value. A second-order account of the hydrostatic state set out by Birch (Ref. 30) gives for aluminum good agreement with the hydrodynamic Hugoniot obtained in other ways (Ref. 25). We can have some confidence therefore in estimating the longitudinal elastic modulus from the same analysis. However, the difference in the rear surface motion arising from the different estimates of the longitudinal modulus is in this case insignificant.

There are other features of the constitutive equation which this experiment leaves unresolved. The rear surface motion is not sensitive enough to the hardening of the material with plastic work for any conclusions to be drawn on this point. Also the plastic unloading behavior is defined only within coarse limits. If the plastic unloading adiabat is taken as $\sigma = P - 2.76$ kb instead of $P - 1.84$ kb but the constitutive equation is otherwise taken the same as Figure 4, the calculated rear surface motion still agrees with the observations to within the experimental error. The variation in rear surface motion due to yield stress variations must after all lie within the limits set by the extreme cases of zero yield stress (hydrodynamic response) and infinite yield stress (elastic response). These are shown in Figure 5; the insensitivity is evident.

There are two ways of looking at this situation. In one respect it is encouraging for it means that at these low pressures, namely less than 10 kb, a simple engineering theory will adequately describe many features of the behavior. In its simplest form such a theory would ignore rate effects, hardening, thermodynamic effects and nonlinear effects of finite strain; the constitutive equation would be piecewise linear. The rear surface motion given by this engineering theory, shown in Figure 5, is clearly a very good estimate.

This calculation was performed graphically by a characteristics method.

From another point of view it is not encouraging. Good though its predictions are in this case, the engineering theory may nevertheless be used only for those situations for which it has been shown to be valid. Its use is justified for some uniaxial strain configurations of practical interest (and these may involve spalling) but we cannot use it with confidence to predict behavior of configurations significantly different from those tested, nor does it enable us to extrapolate to conditions beyond those for which its applicability has been verified experimentally. Furthermore the insensitivity of the observable effects to important changes in the hypothetical constitutive equation means simply that there are aspects of material behavior which cannot be determined in this way.

SECTION IV: THE OUTLOOK FOR FUTURE WORK

On the experimental side some progress can be expected from improved instrumentation techniques. Much more progress is likely to come however from experiments which better exploit the present capabilities of the instrumentation. Such experiments, designed to investigate particular aspects of material behavior, might well depart from the uniaxial strain configuration. Fowles' experiment on the elastic-plastic behavior of aluminum (Ref. 26) in which an oblique shock wave traverses the polished face of an aluminum wedge, his attenuation experiment (Ref. 31) which also employs a wedge geometry and Al'tshuler's experiment (Ref. 5) on the velocity of the first release wave, mentioned above, illustrate the point.

Development of experimental techniques is also required to extend the exploration of material properties to higher loading intensities, to unloading and tensile behavior. Once again it may be advantageous to depart from the uniaxial strain configuration, as has been done for example by Al'tshuler to achieve higher pressures (Ref. 32).

Without any changes in technique however, the possibilities for useful future experiments are endless. There are as many Hugoniot curves to be determined as there are materi-

als, and one can visualize the elucidation of the yield process requiring as many plate impact experiments as it has already required static tensile tests.

On the theoretical side an important objective is the complete mechanical and thermodynamic description of elastic-plastic wave propagation in terms of finite strain theory. It is clear from the discussion above that more evidence from fundamentally different experiments is needed to narrow the limits on the constitutive equations; progress in the theory can be expected to follow the generation of such evidence. Further, a completely satisfactory theoretical description must be based on a satisfactory hypothetical model for the physical processes taking place. Perhaps this is our ultimate objective.

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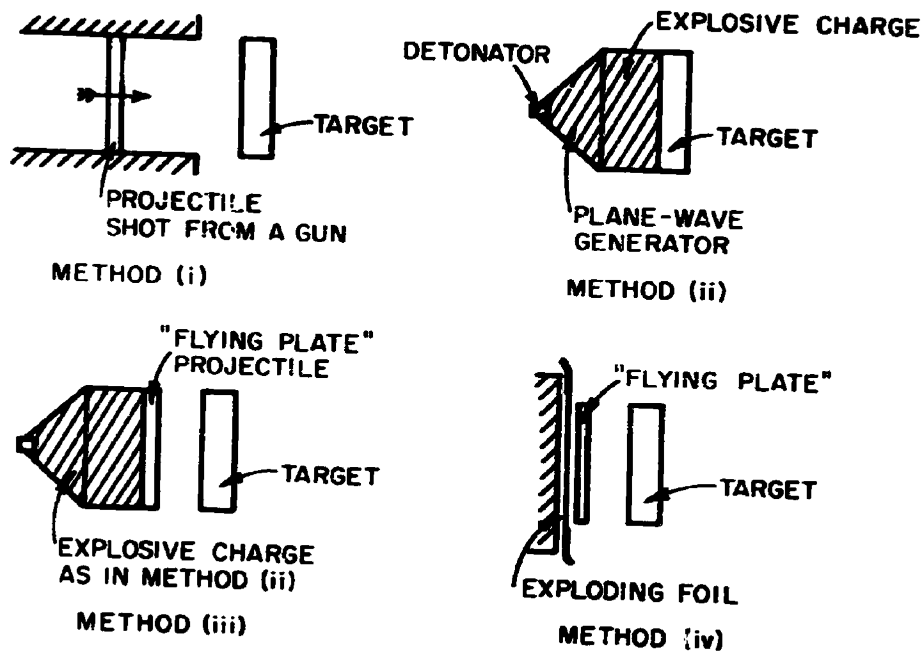

Figure 1. Diagrammatic representation of four methods used to induce longitudinal plane waves in a target plate.

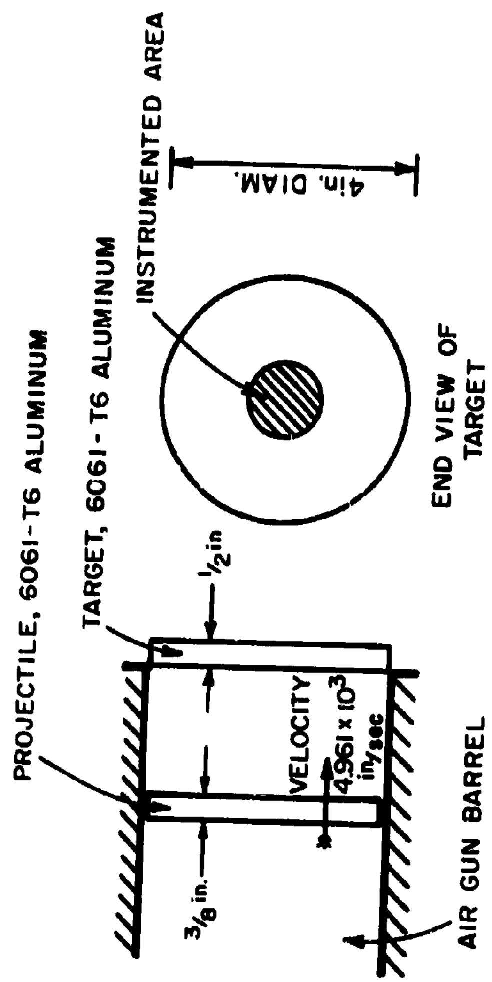

Figure 2. Diagrammatic representation of the plate-impact experiment discussed in Section III.

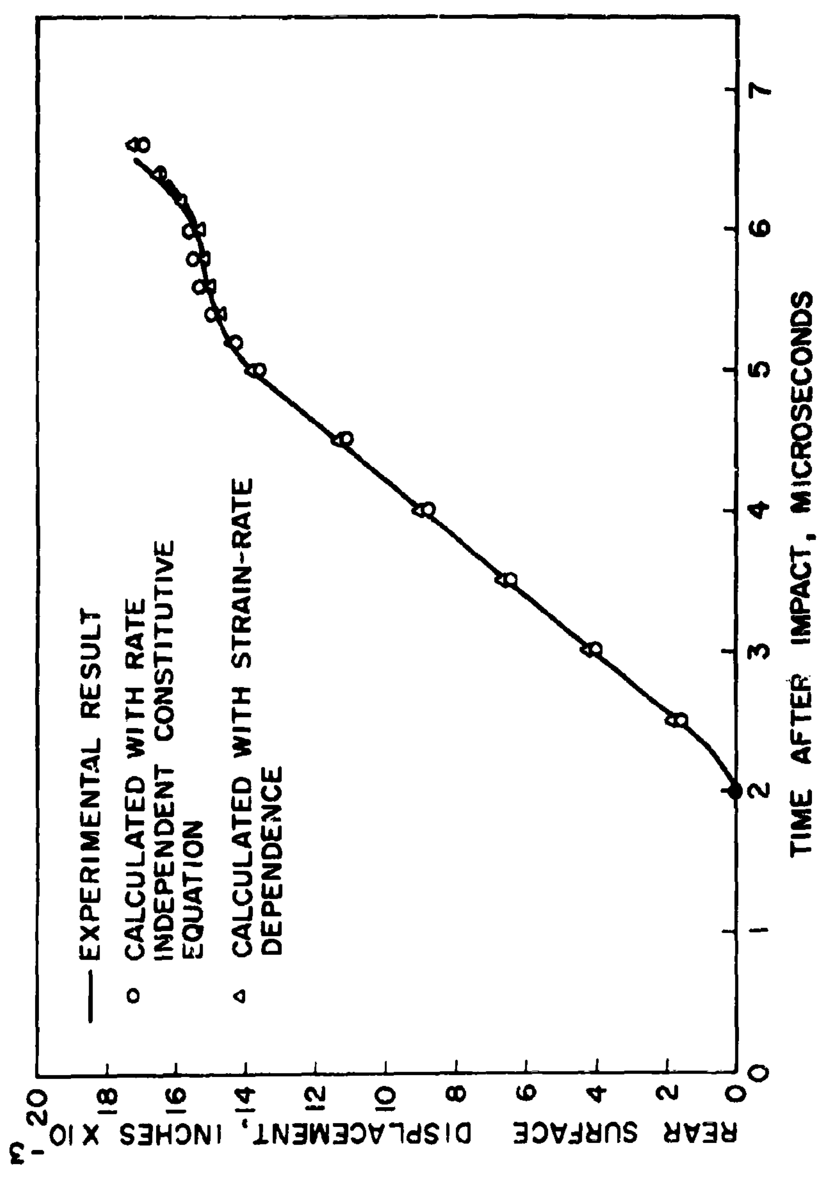

Figure 3. Calculated and observed rear surface motion for the plate-impact experiment shown in Figure 2.

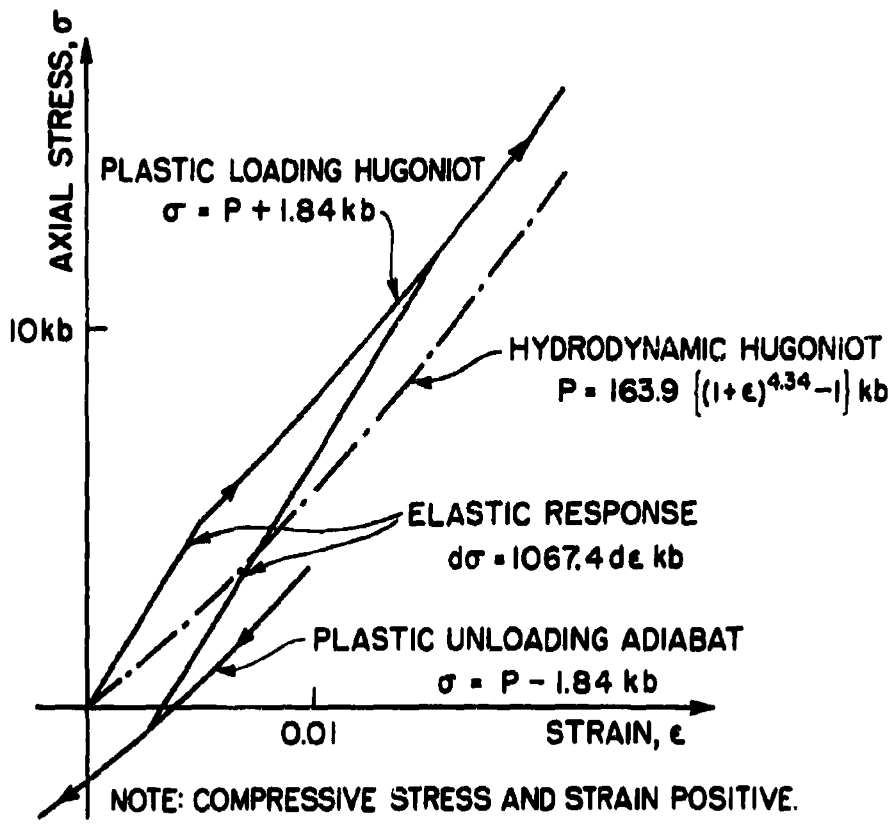

Figure 4. A constitutive equation for 6061-T6 aluminum in uniaxial strain

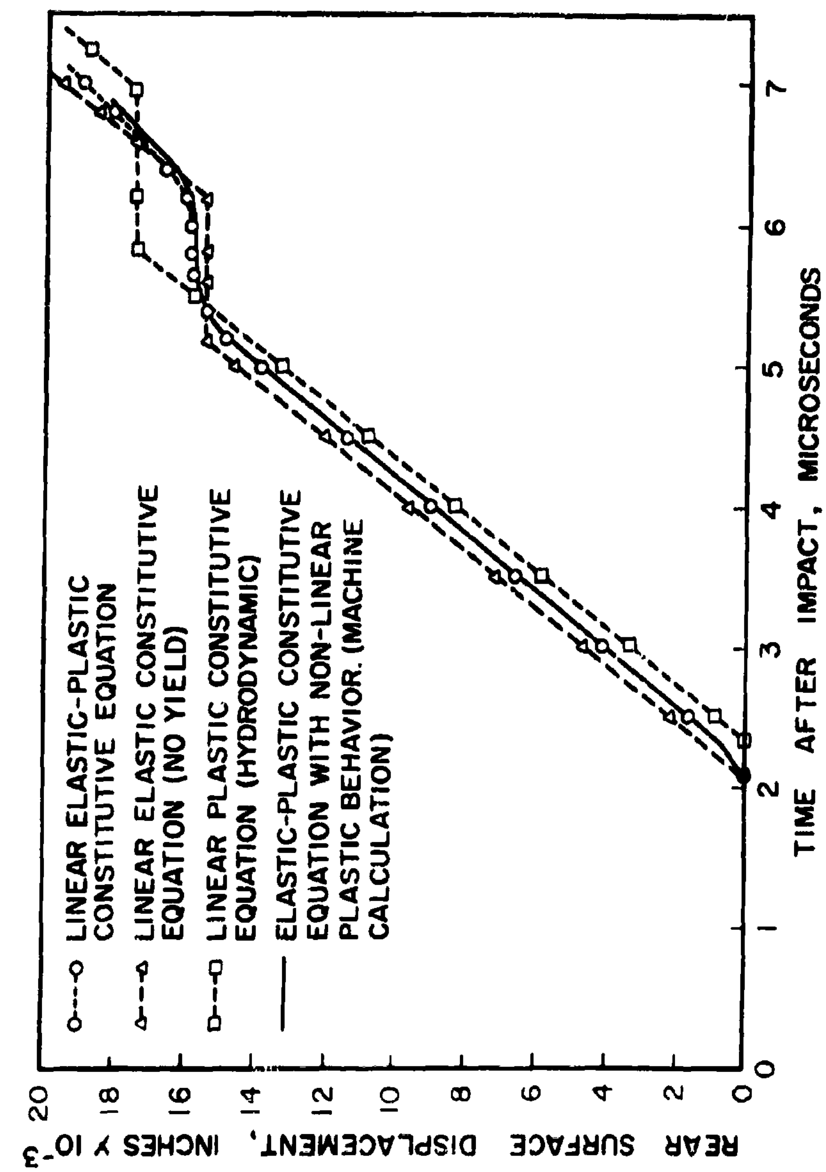

Figure 5. Rear surface motion for the plate-impact experiment shown in Figure 2, calculated according to various hypotheses.

DISCUSSION

DR. GOLDSMITH

Dr. Percy's paper is now open for discussion.

W. O. DAVIS, HUYCK CORPORATION

When I was at Los Alamos, I was always rather deeply disturbed by the fact that although we found engineering solutions, and sometimes even empirical equations to handle some of the problems we have, we never really faced up to the fact that we were probably violating Newton's second law most of the time. Have you given any serious consideration to the macroscopic behavior of these impacted plates? In other words, you measured the motion in the front surface. Have you also measured the motion of the rear surface or computed the motion in the center of gravity in response to the applied force?

DR. PERCY

Well I guess most of the second laws are violated in our analysis, and well, all I can say is I have every confidence that if written correctly our conservation equations are correct although we haven't looked at the plate as a whole. We should in fact have a model which will probably add up.

FROM THE FLOOR

May I say that I have analyzed a great deal of other people's data with exactly this in mind. And where they have been concerned with microscopic phenomena, I have integrated their data to see how well they stood up in the macroscopic, and some of the deviations you find are astonishing. I know one very reputable organization who failed to conserve momentum by a factor of 70 percent. I think it might be of some value to examine our basic postulates somewhat more fundamentally. This may be part of the problem in coming up with a theoretical description.

DR. PERCY

I don't think that we have here any really good evidence to reject the hypothesis of conservation, but I don't have any more to add.

SOULES, STANFORD RESEARCH

In comparing the rate dependent and rate independent models one must assume some dependence of the yield stress on the strain rate. Can you tell me what that was. I missed it?

DR. PERCY

Yes I can. We assume that because the yielding process is a thermally activated one, more or less, then, the strain rate is going to be an exponential function of the overstress or the overstress to some power, let's make it one. If I write a Σ_* for the overstress, this is the plastic strain rate which satisfies the strain rate of zero when the overstress is zero. This actually is the function suggested by Los Alamos and the numbers I put on, if you are interested, those numbers were both rounded once;they are just guesses. The 743 has nothing to do with the number of watts in a horsepower.

DR. GOLDSMITH

Any other questions? If not we shall introduce our next speaker who will discuss the subject of Uniaxial Stress Conditions. Dr. Gordon Filbey received his training at Johns Hopkins University in the Laboratory of Professor Bell where I suspect a great many of you know a number of very informative experiments have been conducted on high intensity stress wave propagation. The speaker is at the present time located at the University of Pennsylvania, the Town Scientific School of Civil and Mechanical Engineering. Dr. Filbey, would you give your presentation please.

DR. FILBEY

I would like to begin by saying I admire very much the model that the Committee prepared, sitting out in the lobby. Sorry there are no bars in the model though. Dynamic Conditions of Uniaxial Stress are commonly attributed to the physical situation of longitude and wave propagation rise.

UNIAXIAL STRESS CONDITIONS

by

Gordon L. Filbey, Jr., Ph.D.

University of Pennsylvania

UNIAXIAL STRESS CONDITIONS

Gordon L. Filbey, Jr.

University of Pennsylvania

ABSTRACT

The propagation of waves of plastic deformation is experimentally studied in rods undergoing free flight impact with diffraction grating strain gages. Verification of the strain-rate-independent theory of Kármán and Taylor has been demonstrated for several annealed fcc metals by Bell, using impact velocities up to 1100 inch/sec, and at 3000 inch/sec in experiments of the author with annealed aluminum. Aspects of dynamic overstress, initial development of the plastic wave, final strain distribution in finite rods and unloading phenomenon are discussed.

UNIAXIAL STRESS CONDITIONS

1. INTRODUCTION

Dynamic conditions of uniaxial stress in continua are commonly attributed to the physical situation of longitudinal wave propagation in rods. The experimental results presented here are those of finite amplitude longitudinal waves involving plastic strains up to 12% produced by a symmetrical free flight axial collision in cylindrical metal bars. An accurate and detailed deformation history of the propagating plastic strain wave is experimentally obtained by the diffraction grating technique developed by BELL (1956)(1958) for this purpose. Results from over 500 free flight tests in several metals have shown that there are three phases of plastic wave propagation in annealed face-centered cubic metals. Phase I involves the development of the plastic wave near the impact end of the rod and in the region extending one rod diameter from the impact face. Aspects of this development are explained in terms of initial shock waves by BELL (1960b)(1961a)(1962a), using data obtained with impact velocities from 400 in/sec to 1000 in/sec. Using an impact velocity of nearly 3000 in/sec, I have shown in FILBEY (1961) the transient existence of a different deformation process in the first diameter region, not observed at the lower impact velocities.

Following the initial development of the plastic waves in Phase I, where completeness is lacking in the total description, in Phase II plastic waves propagate in a manner in complete agreement with the strain rate independent finite amplitude wave theory of KARMAN (1942) and TAYLOR (1942) referred to hereafter as the K-T theory. That is, for any given impact velocity, not only strain maxima but also wave velocities observed are in accord with the K-T theory. These results hold for the tests of FILBEY (1961) in annealed aluminum as well as for each one in the extensive number conducted by BELL in annealed aluminum, copper and lead. Observed relationships and balances on final deformation energy to initial kinetic energy of BELL (1960b) led me to observe that, within the framework of the strain rate independent K-T theory and for a special case which was consistent with BELL's data, the governing stress-deformation law must be in the form of a power law. BELL (1961a) showed that, in fact, all his data fit a <u>parabolic</u> stress-strain law of the form $\sigma = \beta \epsilon^{1/2}$, and <u>a fortiori</u> that the constants involved are relatable to the theory of dislocations in polycrystalline media proposed in the early 1930's by TAYLOR (1958).

In finite length rods, Phase III is the result of the wave reflection process from the free end which controls penetration of the plastic wave into the rod and ultimately stress unloading at the impacted end and separation of hitter and struck specimen. BELL (1961a) (1961b)(1961c) has constructed a model whereby he can predict in the annealed face-centered cubic materials of his experiments, the final strain distribution in the rod, time of contact of hitter and struck specimen, and a coefficient of restitution for symmetrical impact. Aspects of Phase I have controlling influences over Phase III, as will be discussed more fully below. Furthermore, for rods of less than some specified length to diameter ratio, Phase III can prevent any visible evidence after the test of the presence of Phase II. Such evidence does not preclude, however, the general applicability of the K-T theory to plastic wave propagation in rods of annealed fcc metals.

2. EXPERIMENTAL DATA

The deformation waves involved in these tests are initiated by "constant velocity" axial impact. This is achieved by impacting a square-ended round rod with another rod of identical material and dimensions, subjected to the same machining and annealing operations. The hitter is fired from a smooth bore air gun, ported at the end so as to achieve a uniform hitter velocity, and its velocity prior to impact is measured by an electronic timer. The constant velocity of impact, which we shall denote as impact velocity v_0, is by symmetry considerations exactly equal to one-half the hitter velocity. No additional constraints are introduced at the impact plane in the radial direction by this method of wave initiation; only inertial constraints offered by the dynamic deformation process of the material itself can be present. BELL's tests always involve identical hitter and specimen, such that the impact velocity is constant for the entire time interval the rods are in contact. In the case of FILBEY's (1961) experiments, an additional appendage was necessary at the rear of the hitter rod, evident in the schematic of the apparatus in Fig. 1. Since my recording time was limited to the first 10 μsec following impact, this in no way affected the strain-time results. A conservative estimate of arrival time of the first information from the free end of the shorter specimen is 30 μsec. The appendage was required to fire it from the special high velocity air gun provided the author by BRL, Aberdeen Proving Ground, Maryland. In addition, the O ring seals prevented air from leaking past the hitter into the vacuum chamber in which the impact took place in my experiments.

At the position on the specimen (Lagrangian coordinate X in the following) where it is desired to observe the strain-time behavior, 154 lines of a 30,720 line/inch diffraction grating are ruled in the manner of screw threads on the cylinder. The total gage length is

0.005 inches. The grating is illuminated in a uniform parallel monochromatic light field (5461 Å = mercury green line) that is initially normal to the grating surface. For given incident light wave length and initial grating spacing, the diffracted image angles of the two first order lines shown in Fig. 1 are determined by grating rotation (surface angle) and grating spacing variation (strain). For the values mentioned, the two first order lines are the only present, and make initial equal angles $\theta_o = 41^\circ\ 18'$ with the grating normal. Dynamic knowledge of the angular rotations β_A and β_B during the deformation process enables one to determine the strain and surface angle from equation (2.1). These rotations can be detected by masking the cathode faces of photomultiplier tubes with vee shaped openings. To sufficient approximation (BELL (1958), FILBEY (1961)) the following hold:

$$\epsilon = (\beta_B + \beta_A)\frac{\cot\theta_o}{2}$$
$$\alpha = (\beta_B - \beta_A)\frac{\cos\theta_o}{2(1+\cos\theta_o)} \qquad (2.1)$$

The surface angle rotation is given by α, from its initial value of $\alpha = 0$. As will be discussed further below, additional important data is obtained by knowledge of surface angle behavior during the passage of the wave. Lagrangian compressive strain measure is given by $\epsilon = -\frac{\partial u}{\partial X}$, where X is the Lagrangian coordinate of material points along the idealized one-dimensional rod, and u(X) is the physical displacement of material points. Surface angle α is, on the other hand, referred to laboratory coordinates, and appropriate caution must be exercised to relate it to gradient of radial displacement in terms of the Lagrangian length coordinate X (FILBEY 1961). Generally speaking, for small strains the formulae are accurate to within the order of the strain measured.

For the majority of BELL's tests, strain maxima are never more than 2.5% strain. Within the active area of the diffraction grating that is serving as the gage there are 20 or more grains of the polycrystalline aggregate. For strain maxima of this order, surface polish is maintained sufficiently well such that the photomultiplier responses may be used directly in the calibration curve for each grating. Calibration of a grating is accomplished by rotating the incident light beam through known angles γ about the grating surface as center. Using the appropriate transformation to $\beta = \frac{\gamma}{\cos\theta_o}$, a calibration curve of β vs. output voltage is obtained. When larger strains are present, as in the high velocity tests, there is large grain rotation of individual

grains within the aggregate, and the specimen surface assumes an orange-peel appearance during the passage of the deformation wave. The additional photomultiplier used with the beam-splitter in Fig. 1 detects ambient intensity changes from the orange peel such that an appropriate correction can be made.

The basic data provided by such tests is thus strain vs. time and surface angle vs. time. In light of the following section, we shall choose to interpret this as arrival time of each strain level ϵ at station X, and of the corresponding surface angle at the same instant. Some averaged data for several locations near the impact end of 1100 F (99% Al) annealed aluminum rods are shown in Fig. 2, for the high velocity tests. These tests are not a typical example of the data reported elsewhere by BELL (1960a)(1960b)(1961a). In the latter case very flat strain-time maxima are recorded after the passage of the plastic wave. In addition, for a given impact velocity, a constant value of the maximum strain is observed between one and four diameters in 10 inch long, .990 inch diameter specimens. The strain maxima are in agreement with measured values of specimen diameter after the test, assuming incompressibility.

3. DISCUSSION OF RESULTS

The one-dimensional strain rate independent K-T theory predicts that each level of strain propagates with a constant velocity $C_p(\epsilon)$ determined from the slope of the governing stress-strain relation $\sigma(\epsilon)$ by (3.1) and that a relation (3.2) exists between particle velocity and the corresponding strain

$$\rho_o {C_p}^2 = \frac{\partial \sigma(\epsilon)}{\partial \epsilon} \tag{3.1}$$

$$\dot{u} = \int_o^\epsilon C_p(\epsilon) d\epsilon \tag{3.2}$$

The stress-strain relation $\sigma(\epsilon)$ is the one that governs the wave propagation, and not necessarily the static one. Although good agreement was found by BELL (1960a) using the annealed aluminum static curve in (3.1) and (3.2), BELL (1961a) later reported the parabolic stress-strain relationship (3.3)

$$\sigma = (5.60 \times 10^4)\ \epsilon^{1/2} \tag{3.3}$$

The parabolic law (3.3) gives best overall fit with (3.1) and (3.2), as well as other aspects of the theory involving prediction of initial stresses at the impact face and unloading behavior. BELL found it necessary to introduce the parabolic single crystal data of POND and HARRISON (1958) associated with duplex slip into the TAYLOR theory of the polycrystalline aggregate in order to determine a theoretical dynamic polycrystalline stress-strain curve. He also discusses in the (1961a) paper the remarkable agreement when the uniaxial components of TAYLOR's static single-slip data are replaced by their deviatoric components, in going to the polycrystalline aggregate. The stress-strain curve for annealed aluminum, as determined from static compression tests, is given by the broken line in Fig. 3. Equation (3.3) is given by the solid line in Fig. 3. A power law fit good to within 200 psi to 7-1/2% strain is written over the broken line in Fig. 3. The nearly cubic stress-strain relation shown in Fig. 3, (the solid line much higher up), was determined from experimental data given in Fig. 4 for the high velocity experiments of FILBEY (1961) in the first one-half diameter region. In Fig. 4 is shown data that offers one of the substantiations of the K-T theory. The x's along the quadratic curve are obtained from averaged arrival times at all positions between one and four diameters in the 0.990" diameter tests of BELL. Over 200 tests are involved in obtaining these average values. Impact velocities used varied from 400 to 1000 in/sec. The computed $C_p(\epsilon)$ curve from (3.1) are shown by solid lines for the quadratic curve (3.3) and for the static curve of Fig. 2. The static curve computed C_p was included to demonstrate that the difference is measurable. Shown above the quadratic curve in Fig. 4 are the experimental points I obtained in the high velocity experiments, with $v_o = 2794$ in/sec to within 3% on every test. These points were computed from the data shown in Fig. 2. It is remarkable that these waves are also propagating in K-T fashion, i.e., $C_p(\epsilon)$ is constant. This data is entirely unlike initial region data obtained by BELL (1961a)(1962a), where evidence for an unstable shock to 3/4 diameters is presented on the basis of extremely rapid transit times between 1/4 and 1/2 diameter. Through my points in Fig. 4 an empirical fit was made to determine $C_p(\epsilon)$. From equation (3.5) below, the nearly cubic $\sigma(\epsilon)$ relation of Fig. 3 was constructed. If the stress-strain law assumes the form

$$\sigma = \beta\epsilon^{a} \tag{3.4}$$

then from (3.1) one finds

$$C_p(\epsilon) = \sqrt{\frac{\beta a}{\rho_o}}\ \epsilon^{\frac{a-1}{2}} \tag{3.5}$$

has from (4.2)

$$P(\hat{\epsilon}) = \left(\frac{2}{a+1}\right)\left(\frac{2a}{a+1}\right)^{\frac{a}{a+1}} \hat{\sigma}(\hat{\epsilon}) \qquad (4.3)$$

where $\hat{\sigma}(\hat{\epsilon})$ is the corresponding maximum stress of the K-T theory. For the parabola, the dynamic curve so obtained is also parabolic, and is given by $P(\hat{\epsilon}) = 1.164\ \hat{\sigma}(\hat{\epsilon})$. The fit is reasonable above a certain strain level, which is determined from the von KÁRMÁN critical velocity in tension for annealed aluminum.

It was, in fact, this agreement, which has also been shown in copper, that gave original impetus to my studies at the higher velocity. Piezoelectric crystal waters were introduced at the impact face to determine the stress levels in these latter experiments. In Fig. 8 is shown the initial stress pulse in two tests, and in Fig. 9 the stress-time history at the impact face up to time of unloading at 190 μsec in the 4 inch long specimen. It was originally thought (FILBEY (1961)) that the magnitude of the first stress peak of Fig. 8 presented conclusive evidence for the transient existence of the nearly cubic stress-strain law, but a correction in calculation now shows that the stress peak based on (4.3) and the nearly cubic law should be 84,000 psi instead of the 100,000 psi originally reported. Additional tests are being run to see if this discrepancy is fundamental or if many tests average out to the correct value. It should be mentioned that the crystal technique lacks the reproducibility of the grating method. The single test shown in Fig. 9 which was chosen as representative, has several notable features. The first is the comparatively rapid collapse to a stress value that is predicted by the K-T theory for this impact velocity using the parabolic stress-strain relation. In the 800 in/sec tests of BELL (1961c) such flats are evidenced in only very high length to diameter ratio rods (20:1), whereas this rod is less than 5:1. Secondly, there is the JWC overstress close to the calculated value from the parabola and (4.3) of 22,200 psi. In addition, the time of unloading is consistent with BELL's model for the parabolic stress-strain law. Hence, there is additional evidence that the parabolic stress-strain relation and the K-T may be extended to the 12% strain region in annealed aluminum, within the context that one allows for an initial development region.

One remaining aspect of the initial development of the plastic wave is indicated by observed surface angle behavior in the one to four diameter region. It is observed that the surface angle reaches a maximum value at the corresponding time of the arrival of the strain level ϵ, associated with half the total deformation energy, i.e.,

and from (3.2)

$$v_o(\hat{\epsilon}) = \frac{2}{a+1}\sqrt{\frac{\beta a}{\rho_o}}\ \hat{\epsilon}^{\frac{a+1}{2}} \tag{3.6}$$

where $\hat{\epsilon}$ is the maximum expected strain of the K-T theory. A plot of (3.6) is given in Fig. 5 for the above mentioned stress-strain curves. The excellent agreement of the experimental points in Fig. 5 for strain maxima obtained away from the initial development region offer the second substantiation of the theory. The point on the quadratic curve in Fig. 5 for $v_o = 2794$ in/sec was not, however, obtained from grating measurements although all the others were, but only on the basis of before and after impact diameter measurements in several 4" and 10" long specimens. There is, however, sufficient additional evidence that it is correct, which is offered in the next section.

4. THE DYNAMIC OVERSTRESS

Load bar experiments which determine stresses at the impact end of the struck specimen were performed by JOHNSON, WOOD and CLARK (1953). From these tests they constructed a "dynamic" stress-strain curve, shown in Fig. 6, which lies considerably above either the parabolic or static stress-strain curve. BELL (1960b) has shown that by postulating the existence of a nondispersive shock in the neighborhood of the impact face, that brings the media to rest behind the shock for the symmetrical impact case shown schematically in Fig. 7, the JWC dynamic stress levels can be predicted from the parabolic stress-strain law above a certain strain level. If P is the shock stress level, and ϵ_o the shock strain level, mass and momentum jump equations give

$$\frac{1}{2} P(\hat{\epsilon})\epsilon_o = T_o = \frac{1}{2}\rho_o v_o^2(\epsilon) \tag{4.1}$$

where T_o is the kinetic energy per unit volume prior to impact. For nondispersive waves, the energy of deformation behind the shock is equal to the initial kinetic energy, and hence with (4.1)

$$\frac{1}{2} P(\hat{\epsilon})\epsilon_o = T_o = \int_0^{\epsilon_o}\sigma(\epsilon)d\epsilon \tag{4.2}$$

where $\sigma(\epsilon)$ is the material stress-strain law (e.g., the parabola), and $\hat{\epsilon}$ denotes the maximum expected strain from the K-T theory for the impact velocity v_o, from equation (3.2). For any power law (3.4), one

$\int_0^{\epsilon_1} \sigma d\epsilon = \frac{1}{2}\int_0^{\hat{\epsilon}} \sigma d\epsilon$. To a reasonable approximation, evidenced by the difference between O's and B's of Fig. 6, $\sigma_1(\epsilon_1) = \frac{2}{3} P(\hat{\epsilon})$. In addition all strains downstream in the range $0 < \epsilon \leq \epsilon_1$ appear to develop from an infinite step at the origin. Since 2/3 $P(\hat{\epsilon})$ represents the deviatoric component of the initial shock stress, BELL has suggested an equipartition of energy between hydrostatic and deviatoric deformation; and that side wall reflection in the initial region delays the development of the upper part of the wave associated with the hydrostatic position.

5. UNLOADING PHENOMENON

A gross observation of the effect of the free end in the symmetric constant velocity impact is the time of contact between hitter and specimen. BELL (1961b)(1961c) has given a method in which he finds for the time of contact

$$T_c = \frac{L}{C_p(\frac{\hat{\sigma}}{2})} + \frac{L}{C_o} \tag{5.1}$$

where L = length of specimen, $C_p(\frac{\hat{\sigma}}{2})$ is the plastic wave velocity associated with one-half the maximum stress, and C_o is the elastic bar velocity. Further considerations allow him to find velocity at the free end, final distribution of plastic strain, and coefficient of restitution of the struck specimen. Set

$$\left[C_p(\frac{\hat{\sigma}}{2}) \right]^{-1} = \frac{T_c^{(P)}}{L} \tag{5.2}$$

which represents the time of contact per unit length of specimen associated with the hypothetical arrival at the free end of the plastic strain accompanying 1/2 the maximum stress level. A plot of (5.2) vs. impact velocity v_o is given in Fig. 10 for the stress-strain curves considered here. On the right hand scale of Fig. 10 are given T_c values from (5.1) for a 10 inch rod which has a bar velocity C_o of 200,000 in/sec. The model works well above the KARMAN critical velocity, as evidenced by the experimental points of BELL (x's) and FILBEY (Δ's). BELL (1961c) discusses the situation below this impact velocity. Presence of the dynamic overstress at the impact face at a time when unloading would occur by (5.1) tends to lengthen time of contact. Hence for shorter specimens, where there has not been sufficient time for decay of the overstress, time of contact is longer

than given by (5.1). Additional experiments of BELL (1962b) present data for short specimens.

6. CONCLUSIONS

Evidence reviewed in this paper shows that the K-T theory with the parabolic stress-strain law gives an overall description of the dynamic plastic wave deformation process in annealed rods of face-centered cubic metals. There is sufficient data to expect this for impact velocities up to 3000 in/sec. Extension of these ideas, not involving the introduction of a phenomenological strain-rate dependent law, may be used to correlate dynamic overstress with the theory, and unloading behavior.

The initial region behavior in rods undergoing 2800 in/sec constant velocity impact is different from that of the lower velocity impacts. The data indicates that an apparent nearly cubic stress-strain law governs the propagation of the initial wave within the structure of the K-T theory. In view of all other data it is now not clear if this is a material effect or the consequences of a three-dimensional boundary value problem in dynamic plasticity.

In fact, a clear picture of details of the dynamic overstress and its collapse will only be possible when we understand more of the interrelated roles of hydrostatic and deviatoric deformation and of the behavior of plastic waves at free boundaries. It is my feeling that we must establish next results, hopefully similar to the above, for a known experimental situation in which dynamic hydrostatic components are eliminated. The next step would then involve a study of reflection of these waves at free boundaries. Thermal effects have not been included or even mentioned anywhere above; yet they too are inherently present in any plastic deformation process and should be the object of future researchers.

ACKNOWLEDGMENTS

I wish to thank Professor J. F. Bell for the opportunity of spending several years in his laboratory and for his permission to freely discuss his results here. This work and the high velocity work described herein was conducted under the sponsorship of the United States Army, Aberdeen Proving Ground, Ballistics Research Laboratories.

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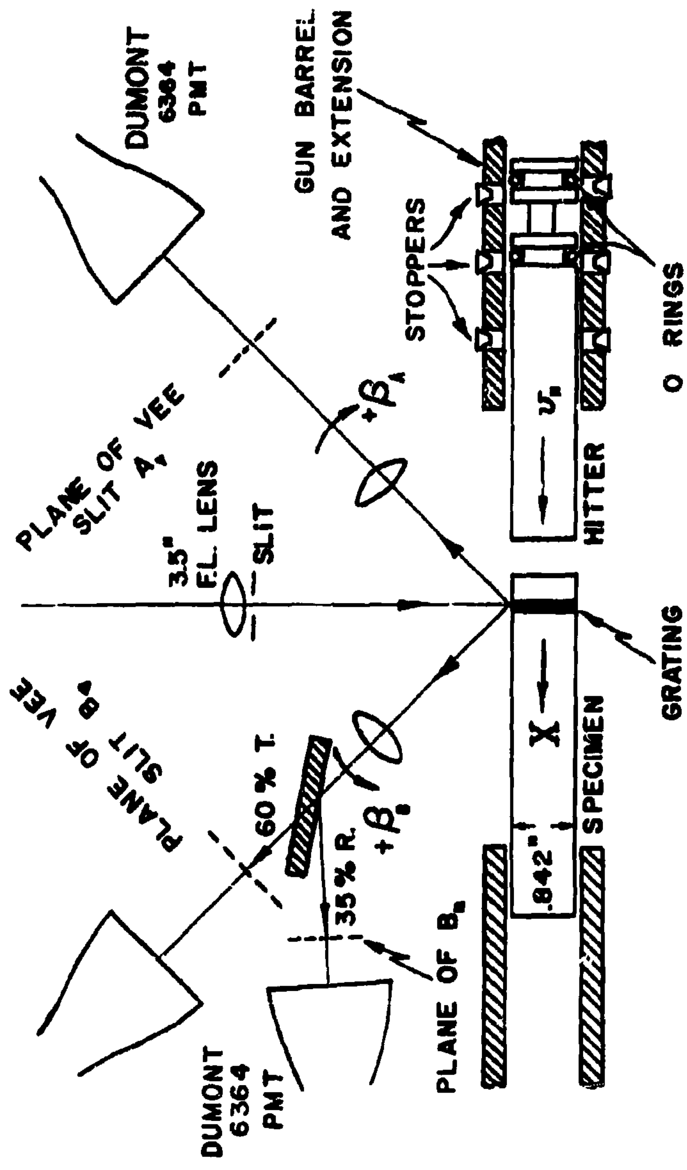

Figure 1. Impact Apparatus and Associated Instrumentation

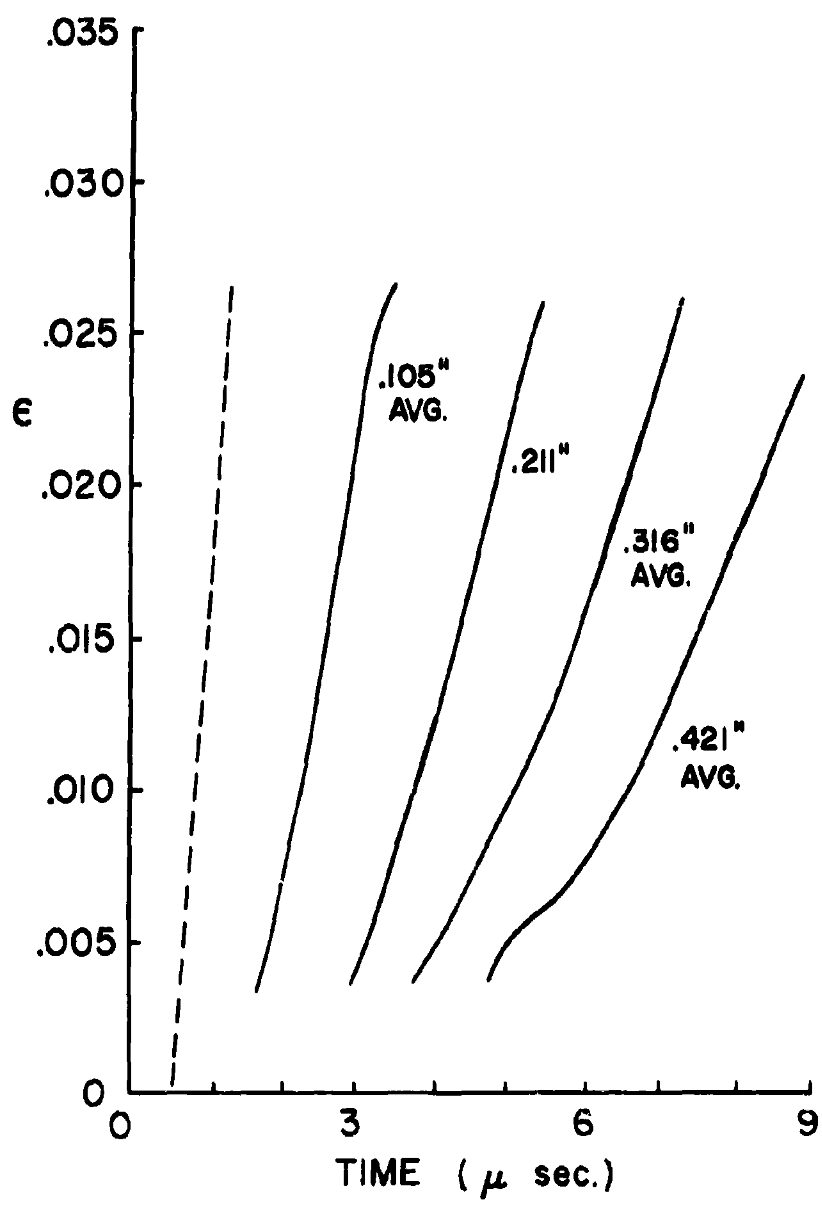

Figure 2. Strain vs Time Data for Aluminum Rods

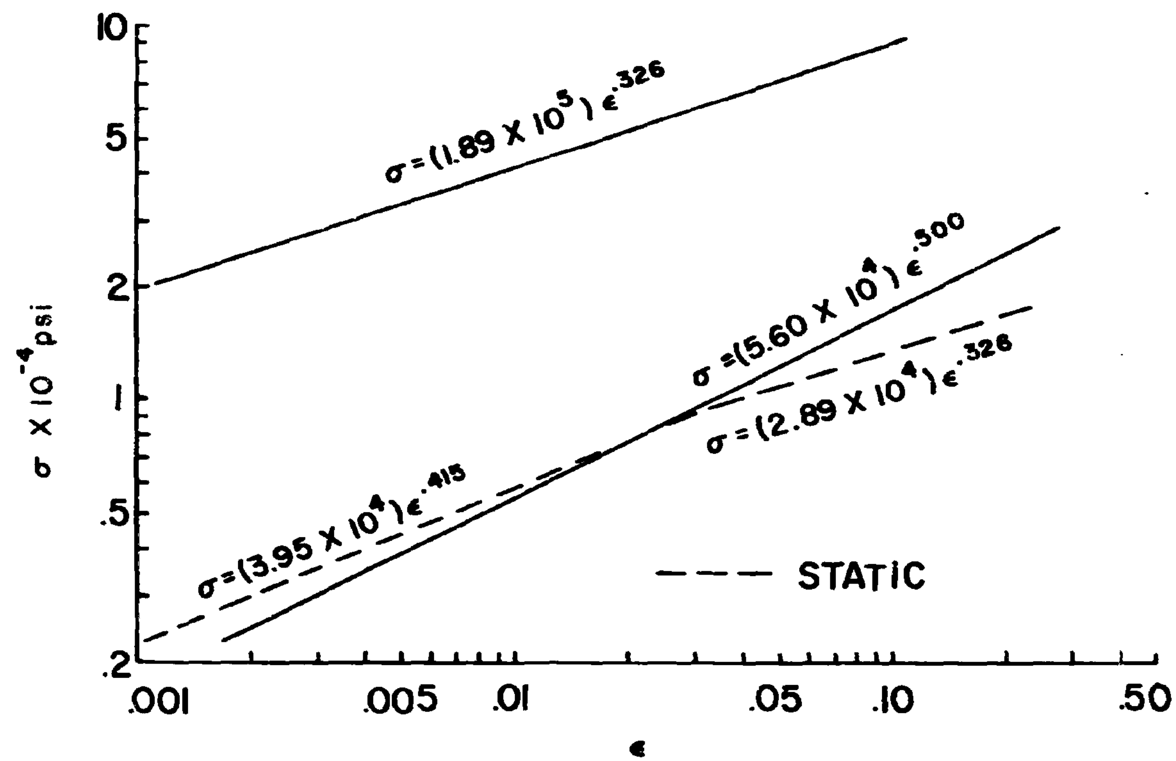

Figure 3. Stress-Strain Curve for Aluminum

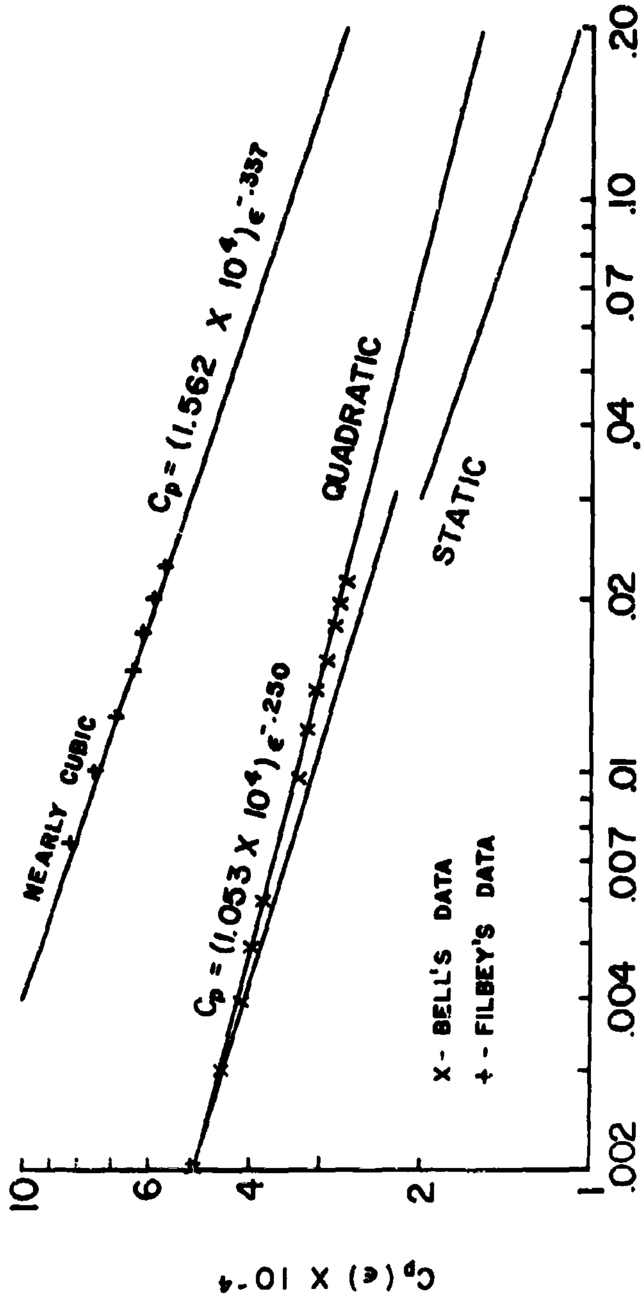

Figure 4. Wave Propagation Data for Aluminum

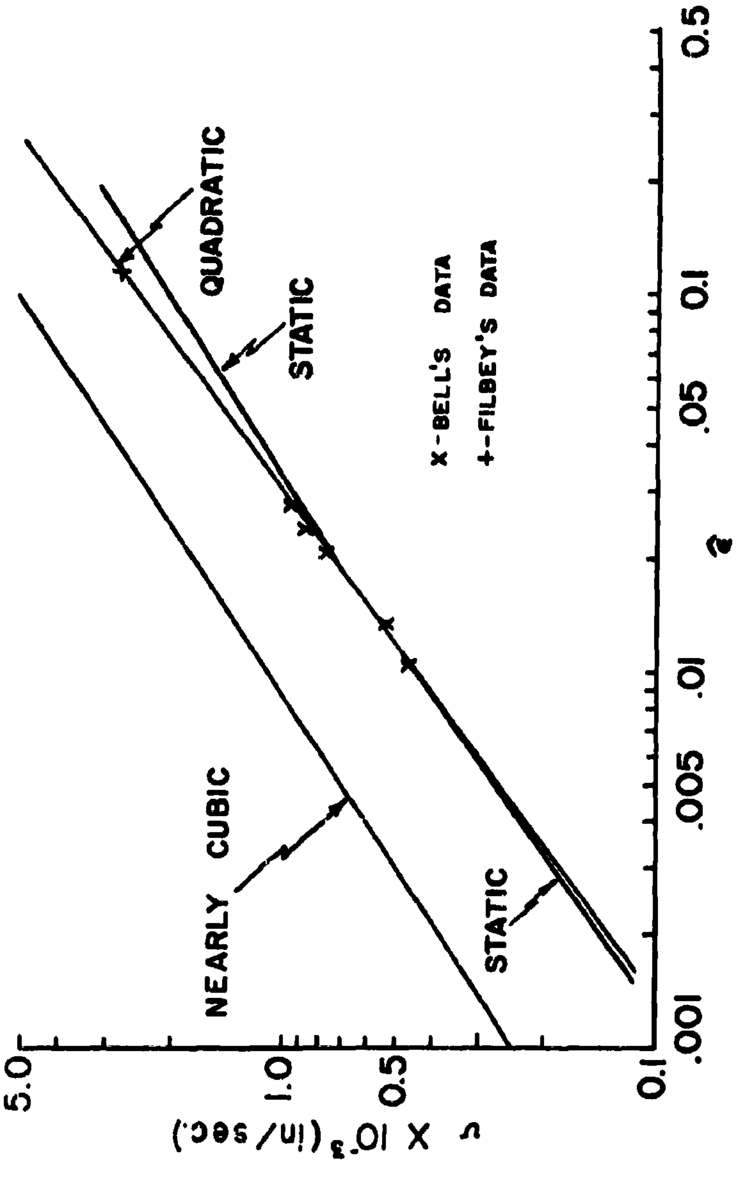

Figure 5. Impact Velocity vs Maximum Strain

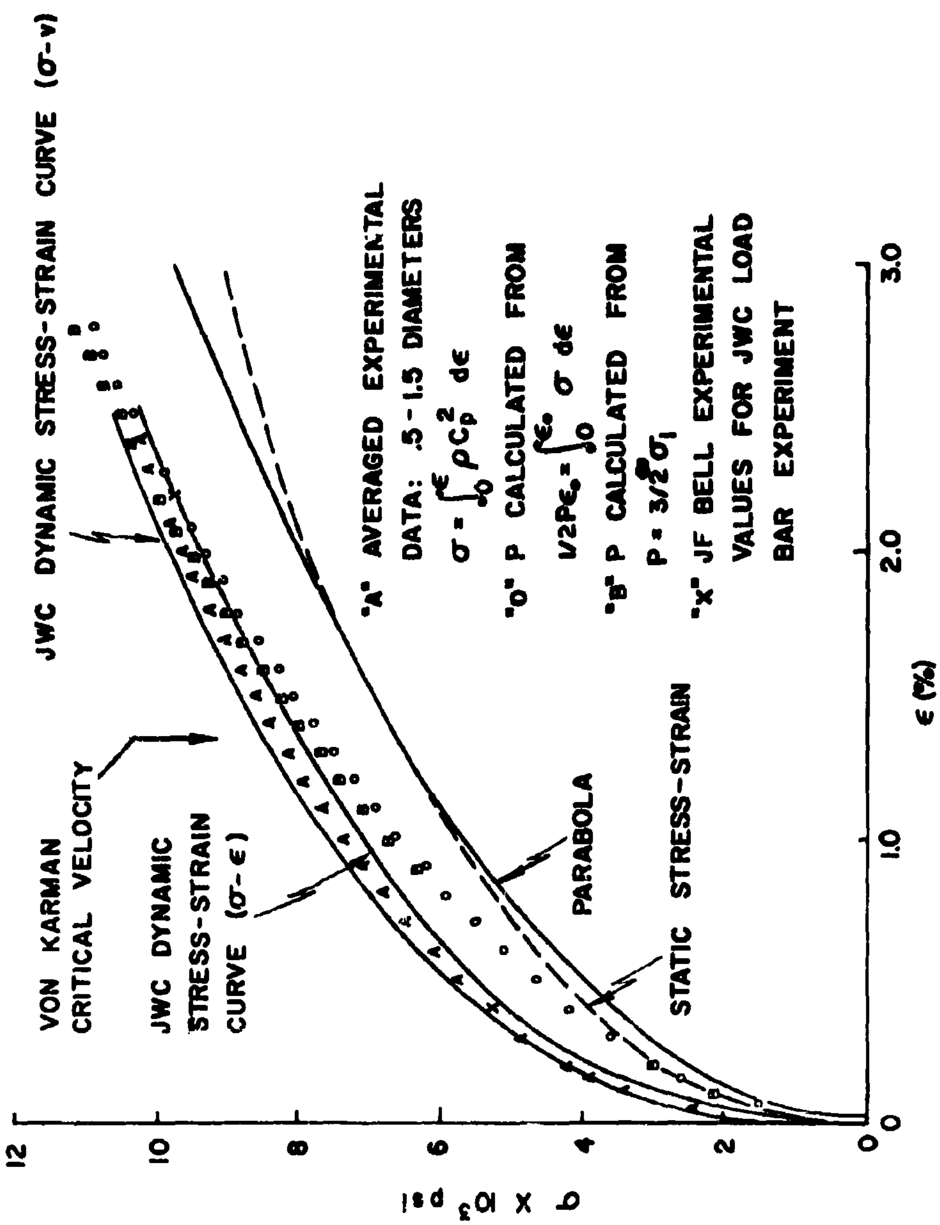

Figure 6. Stress-Strain Curve as Determined for Impact Stresses in Bar Specimen

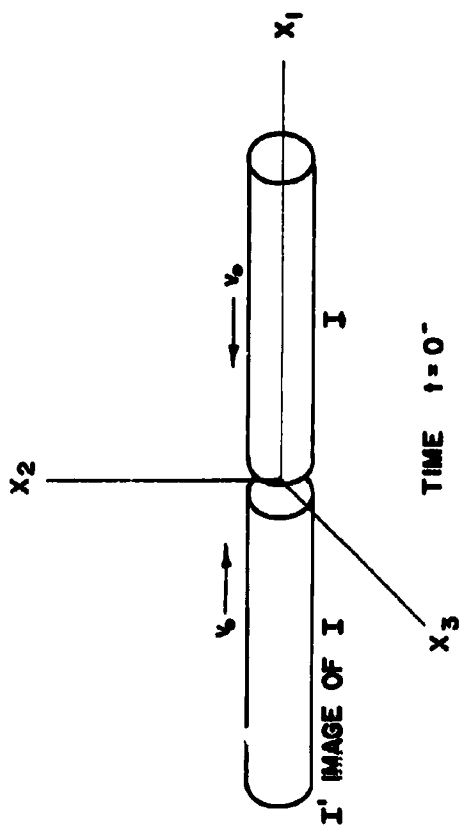

Figure 7. Symmetrical Impact Case

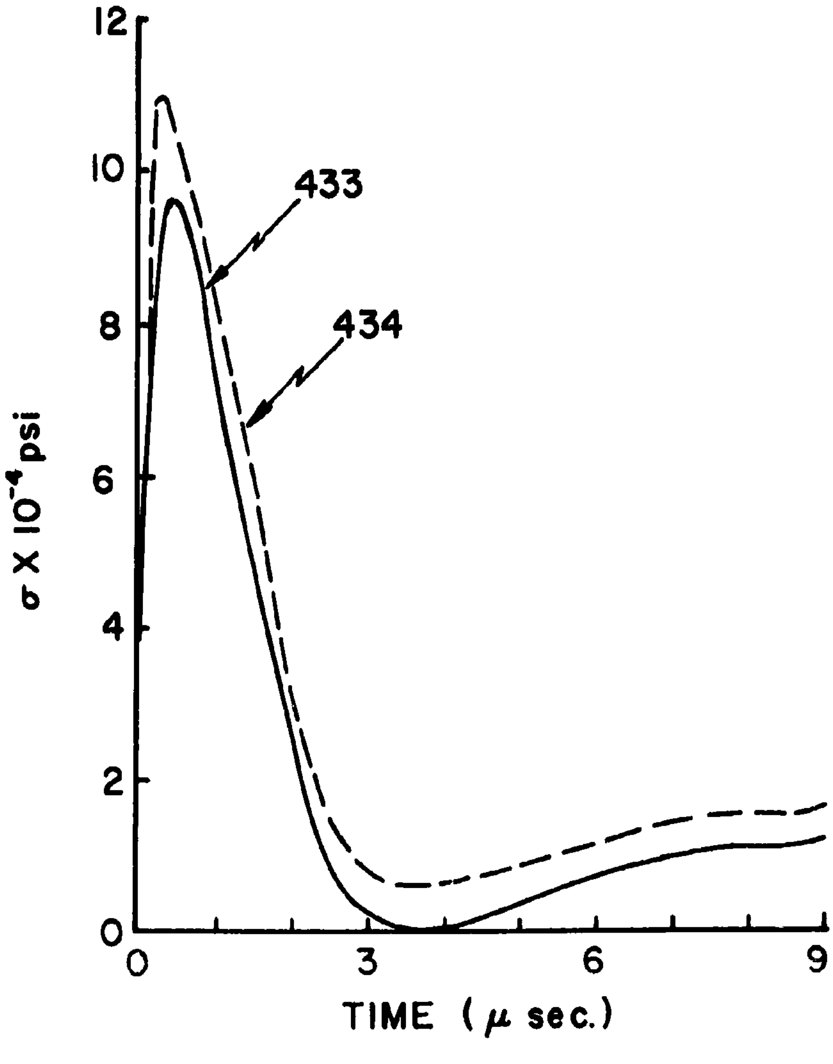

Figure 8. First Stress Peak

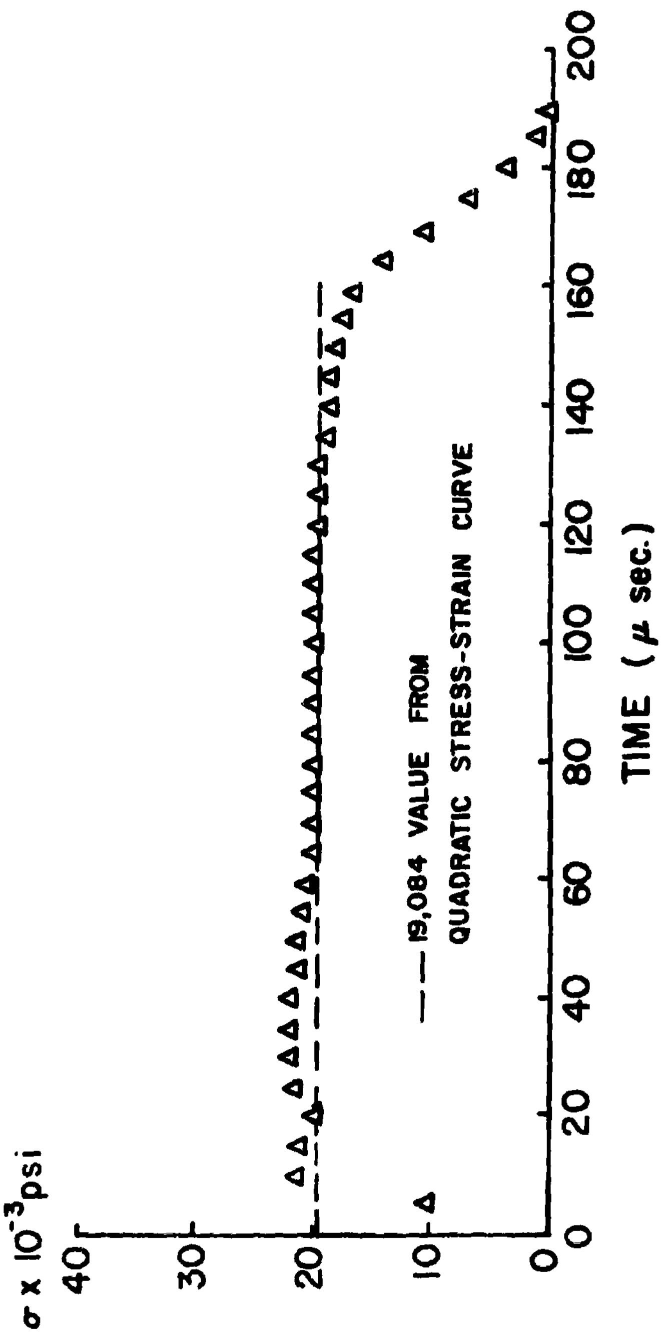

Figure 9. Crystal Data

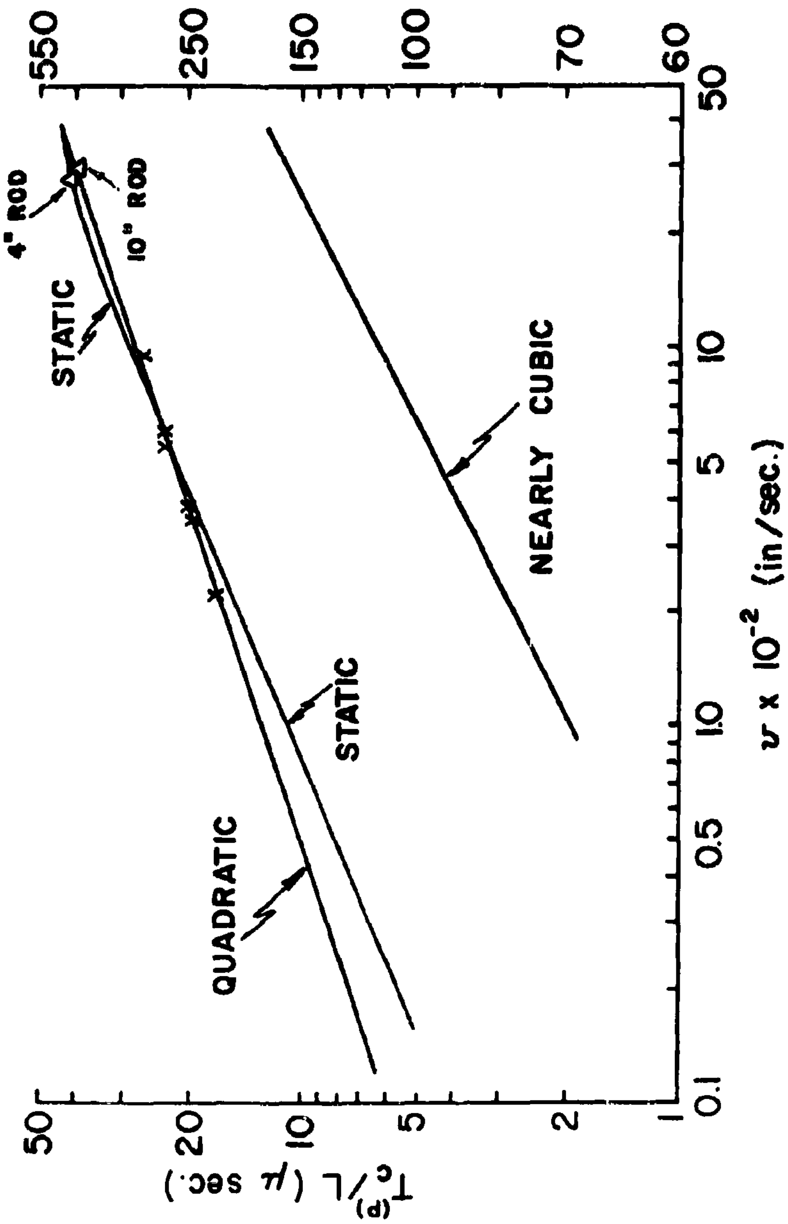

Figure 10. Plastic Wave Velocity vs Impact Velocity

DISCUSSION

DR. GOLDSMITH

I am aware of the fact that the presentation just completed, intermingled with some of the other presentations that have been given, or will be given, represent a somewhat controversial area; therefore I think you should allow us the maximum time permissible for questions.

FROM THE FLOOR

This is with regard to experimental techniques. There have been many papers (I noted one today also) that give the number of experiments and I wonder if you would comment on this a little for me? We happened to be fortunate enough to get a copy of Dr. Sperrazzo's thesis and it came to our attention there that his averages are actually applied to wave velocities and things like this; there are a great number of points and then a line is chosen to draw through these many points. The spread on these points seems to be fairly large. I wonder, is this because of tilt or experimental variables, and if so, how is this line drawn? How is this average made in the matter of the spread?

DR. FILBEY

I will take your questions in order. I will tell you first about the average. Actually, they are averaged in many ways but--just say you run tests at a certain station (writing on blackboard) "Capital X_o," and this would be strain-time data at that station. Then you average the arrival time between the same number of tests. Now the other point is quite interesting and relates to what Dr. Ericksen was saying this morning. In fact, maybe we have been thinking about plastic deformation as a very homogeneous thing; that is rather a very simple minded fashion. If you knew the tension or compression we would have a homogeneous state of strain in the material. In fact our suspicions are that we don't. Indeed, this seems to be one case where this is holding out. (Points to equation on blackboard). You can't really exactly predict in every situation a real determinant situation. There is just certain indeterminancy, and I think that fact ties up to this, that we have lost homogeneity in our exploits.

FROM THE FLOOR

One other comment that was of interest here concerns the piezoelectric gauges that we used here, the contact time. We have done quite a bit of work on this at Sandia and we are presently using a gauge configuration, in a sense different than you usually think of a quartz gauge. In other words, the usual kind of quartz gauge is a sandwich wafer of some kind in which you establish a steady state stress throughout the quartz and relate this average stress until we finally reached the total charge release. It has been found that by using a thick piece of quartz in which you make your time measurements before the transit time of one wave through the quartz, that you can relate the current output to the innerphase stress. We are currently making measurements on this. If time permits, tomorrow perhaps, Dr. Goldsmith will allow me to say a little more about this.

DR. FILBEY

I think one slight objection to this is that you have lost some impedance match. I actually object to wafer tilt with this radial constraint problem.

DOLES, JOHNS HOPKINS

I think maybe one reason of the scatter of points in Dr. Sperrazza's dissertation is probably due to the difficulty of having large grain size. A single grain of the material may be as large as one station.

DR. FILBEY

I didn't say this. The largest size of a typical grain diameter in the aluminum used may be in the order of five thousandths of an inch and smaller. The active areas of defraction grating as a rule gather light over about 20 grains and 20 grains is not a thousand grains --so this can put some variation in the data tilt.

FROM THE FLOOR

I missed a major point. You said you had evidence that the stress is not strictly uniaxial, so how can you say then that data fits the von Karman theory?

DR. FILBEY

I meant I have evidence the strain is not, but then one can infer that the stresses are not. This is a sort of universal problem in doing a lab experiment. You are making a surface measurement and you know it. In the spirit, however, you replace this rod by a sort of one-dimensional equivalent element and you are given a one-dimensional compliance curve for it, because tests will run in some variation of diameters. Tests have been run with quarter-inch rods, well, let's say half-inch rods for the most part. The data I ran were different from Bell's; mine were a little under seven-eighths. Bell's were 0.990, which is just about an inch and so they fit between these three diameters; you can vary specimen diameters. You can also run tests on hollow tubes which is another geometry variation, and all these fit the von Karman-Taylor Theory. Now the aspect of wave development might be a little bit different, but this C_p of Epsilon and Epsilon minus come out.

FROM THE FLOOR

Have you done any work on materials such as body center cubics where strain effectives are more important?

DR. FILBEY

Some work was done on hexagonal metal by Mr. Conn at Johns Hopkins which had an upward turning stress strain curve. They were hoping to see a shock wave; they didn't.

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DR. FILBEY

Some work was done on hexagonal metal by Mr. Conn at Johns Hopkins which had an upward turning stress strain curve. They were hoping to see a shock wave; they didn't.

But again this unloading from the free end got in there and actually prevented the development of the wavelength. Where you can get away with aluminum using a ten-inch long rod, you can see a plastic wave, essentially it is a semi-infinite rod for about five inches of that. You probably have to use a hundred-inch rod . . . (inaudible) . . . . Part of the choice is soft materials so you don't have to shoot it as hard.

DR. GOLDSMITH

I would like to make just one more comment on Dr. Filbey's discussion. I have been generally bothered in looking at these one-dimensional stress experiments, with respect to the radial constraint that is imposed at the interface because of flecture for one thing and because of radial inertia for another. These factors are not taken into account at least in the basic von Karman-Taylor Theory and these are problems, particularly in the first diameter, which I imagine would be important. Would you perhaps care to comment on that?

DR. FILBEY

Well to the extent that in the experimental situation of impacting identical rods, we feel that this is esthetically the best way to go. Of course, we are well aware of this radial acceleration problem. And, another thing, since you are dealing with dispersive waves let's say a one-to-four-diameter region, your radial accelerations are also varying in that, so why shouldn't the von Karman-Taylor theory work if radial accelerations are of first rate importance? It is still an open question. In trying to start out a shock wave you have reflections from the walls and some way things get straightened out. The same problem is really inherent in elastic-impacting elastic rods; you don't see what you should see, in the first diameter of the rods.

DR. GOLDSMITH

This is why you make your measurements beyond these points.

DR. FILBEY

I would say this is why the theory applies.

DR. GOLDSMITH

Thank you Dr. Filbey for your extremely interesting discussion.

Next on the agenda is the subject of "Dislocation Concepts of Strain Rate Effects." This paper was written by Professor John E. Dorn and Professor Frank E. Hauser; it will be presented by Dr. Dorn. Professor Dorn received his Doctor's degree from the University of Minnesota. He is extremely well known in the field of physical metallurgy, having published well over a hundred papers in the area. He has had many, many years of experience in the subject of dislocation theory, having won a number of awards for his papers and recognition by several agencies for the merits of his work. At the present time Dr. Dorn

is Professor of Metallurgy at the University of California, Berkeley, and I will call on him now to give his presentation.

DR. DORN

Mr. Chairman, lady and gentlemen. A major problem encountered in formulating a realistic mathematical theory of plastic wave propagation concerns the dynamic behavior of materials in the plastic regime. Until recently, the approach to an understanding of the dynamic plastic behavior has been exclusively empirical and experimental. Whereas one group of investigators has continued to exist in conformity with the von Karman-Taylor assumptions that the dynamic stress strain curves are insensitive to the strain rate, other investigators have been strongly convinced that strain rate effects are significant.

DISLOCATION CONCEPTS OF STRAIN RATE EFFECTS

by

John E. Dorn, Ph.D. and Professor Frank E. Hauser

University of California

Dislocation Concepts of Strain Rate Effects

by J.E. Dorn and Frank Hauser

ABSTRACT

Whereas von Karman and Taylor independently based their analyses for plastic wave propagation on the deformation concept which assumes the deformation stress is exclusively a function of the plastic strain, Malvern believed that a visco-elastic type of behavior is more realistic, assuming the flow stress is a function of the strain rate as well as the strain. With relatively few exceptions, current investigators of plastic wave propagation phenomena are rather sharply divided into two camps, those that insist on the von Karman-Taylor type of formulation and those that adhere to the Malvern approach. It is the thesis of this paper that both concepts are substantially correct under appropriate circumstances. This thesis will be upheld not only in terms of new experimental evidence but also in terms of deductions arrived at from elementary dislocation theory.

I. INTRODUCTION

The major problem that is encountered in formulating a realistic mathematical theory of plastic wave propagation concerns the dynamic behavior of materials in the plastic regime. Until recently, the approach to an understanding of the dynamic plastic behavior has been exclusively empirical and experimental.(1) Whereas one group of investigators have continued to insist, in conformity with the von Karman(2) and Taylor(3) assumptions, that the dynamic stress-strain curves are insensitive to the strain rate, other investigators have been strongly convinced that strain-rate effects are significant. In fact a dicotomy of viewpoint has developed between two groups of extremists even though the experimental conditions leading to the two sets of empirical deductions were generally somewhat different. In certain cases, however, the differences in the conditions of test and the materials that were tested appeared to be small.

It is the objective of this summary to reveal that both viewpoints are substantially correct under appropriately different conditions; and in some instances the difference in conditions of the test, differentiating between strain-rate sensitive and strain-rate insensitive results, may indeed appear to be small unless subjected to deeper scrutiny. Our interest in this paper will be limited to crystalline materials, particularly metals; therefore, we shall categorically insist, without further proof, that various permissible dislocation mechanisms are responsible for the plastic deformation. When these mechanisms are thermally activated, the stress-plastic strain behavior will be strain-rate sensitive. Under remaining athermal processes, the stress-plastic strain curve will be strain-rate insensitive.

II. SIGNIFICANCE OF THE STRESS-PLASTIC STRAIN BEHAVIOR

The only difference between various mathematical theories of plastic wave propagation concerns the assumptions that are made regarding the plastic behavior of the material.[4] In order to illustrate this issue we consider the simplified problem of a plastic wave moving along a thin cylindrical rod using, as first formulated by von Karman, Lagrangian coordinates. The two equations that satisfy the conditions of continuity and the conservation of momentum, which are common to all possible theories, are

$$\frac{d\epsilon}{dt} = \frac{dv}{d\alpha} \tag{1}$$

$$\frac{d\sigma}{d\alpha} = \rho \frac{dv}{dt} \tag{2}$$

where

ϵ = the total axial engineering strain

t = the time

v = the particle velocity

α = the Lagrangian positional coordinate

σ = the engineering stress

ρ = the density of the material

The additional equation needed to solve for the three dependent variables of σ, ϵ, and v is a realistic formulation of the plastic behavior of the material. Unfortunately it is not apparent à priori what type of general formulation might be needed. For purposes of discussion, however, we will consider three limiting cases where the time enters

the analysis in three different orders of magnitude, namely

$$d\dot{\epsilon} = p\left\{\sigma, \epsilon, \dot{\epsilon}\right\} d\sigma + q\left\{\sigma, \epsilon, \dot{\epsilon}\right\} d\epsilon + v\left\{\sigma, \epsilon, \dot{\epsilon}\right\} dt \qquad (3a)$$

$$d\epsilon = \frac{1}{E}\, d\sigma + g\left\{\sigma, \epsilon\right\} dt \qquad (3b)$$

$$\sigma \equiv \sigma\left\{\epsilon\right\} \quad \text{or} \quad d\epsilon = f(\sigma)\, d\sigma \qquad (3c)$$

where E is Young's modulus and the test is done at constant temperature.

Eqn. 3a refers to situations where the change in strain rate is significant. An example of such a case concerns the propagation of a shock in stress, which is above the dynamic yield strength, along an originally unloaded bar. Dislocations in the unloaded section of the bar are stationary, and therefore have zero velocity also at the first instant the shock in stress acts on them. But immediately they begin to accelerate. If they are otherwise unimpeded they will soon acquire the velocity of sound leading to extremely high strain rates.[5] Because of the very small inertia of dislocations the acceleration period will occur over about 10^{-10} seconds. Consequently such accelerative phenomenon leading to changes in strain rate will not be detected in experiments where the time is not measured in shorter intervals than 10^{-10} seconds. Since no experiments have yet been made in this regime, we will not dwell longer on Eqn. 3a.

Eqn. 3b is a generalization of Malvern's[6] viscoelastic relationship. Whereas the first term to the right of the equality is the increment in elastic strain, the second term gives the increment in plastic strain, the function $g\left\{\sigma, \epsilon\right\}$ being the plastic strain rate. This formulation of

the plastic behavior is substantially consistent with dislocation theory under conditions where the accelerative phenomena can be neglected. When, for example, an increment of stress $d\sigma$ is applied in $dt = 0$, only an increment in elastic strain results. But as will be shown later, $d\epsilon$ is not an exact differential and the strain-rate function $g\left\{\sigma, \epsilon\right\}$ will depend on the strain-rate history, the physically appropriate variable for g being the substructure in lieu of the strain ϵ. As shown in Fig. 1, if a bar is held for a long time at σ, and then is subjected to an increment of stress $d\sigma$, the path of deformation will be an instantaneous elastic strain from A to B followed by a time dependent straining from B to C. The curve OAC therefore refers to the stress-strain curve conducted at an infinitely slow strain rate.

Eqn. 3c assumes that the stress is exclusively a function of the strain as originally assumed by von Karman and Taylor. It demands that when an increment of stress $d\sigma$ is applied, the stress-strain curve is traced directly from A to C, as shown in Fig. 1. This assumption, as we have seen, is basically inconsistent with dislocation theory since the inertia of the dislocations demands that the true path go from A to B to C. Under certain very stringent conditions, however, the plastic behavior might nevertheless be approximated by such a strain-rate independent phenomenon. If for example the plastic strain rate is so rapid that the plastic straining from B to C takes place over a shorter time interval than the least count of time possible with the experimental conditions employed in the test, the plastic behavior will appear to be strain-rate insensitive.

III. STRAIN-RATE EFFECTS FOR THERMALLY ACTIVATED MECHANISMS

Dislocation mechanisms of deformation can be classified into two major groups, those that are thermally activated and those that are athermally activated. When the energy of the unit mechanism is less than about 50kT the process may be thermally activated. But when the energy required to complete the process is greater than about 50kT, successful thermal fluctuations to aid the process are so infrequent that the process can be made to occur only by mechanical means. The flow stress for thermally activated processes decreases rather rapidly with an increase in temperature, whereas in most cases the flow stress for athermal processes decreases slowly with an increase in temperature in a manner proportional to the decrease in the shear modulus of elasticity with an increase in temperature. Furthermore thermally activated processes always give strain-rate sensitive flow stresses.

Straining results from the slip displacement of dislocations. If the energy required to produce this displacement is high, the process is not thermally activatable. There are a number of such athermal processes, typical examples of which are:

1) Motion of dislocations through long-range back stress fields due to other dislocations,[7]
2) Motion dislocations through very high short-range back stress fields due to other dislocations,[8]
3) Motion of dislocations through alloys having short-range order,[9, 10]
4) Unlocking of Suzuki-locked dislocations.[11]

Other mchanisms have much lower energies and are therefore thermally activatable; typical examples are:

1) Peierl's mechanisms of the forward motion of a dislocation from one energy well to the next, [12, 13, 14]
2) The intersection of two dislocations, [15, 16, 17]
3) Cross-slip in face centered cubic metals, [18]
4) Motion of jogged screw dislocations leading to formation and diffusion of vacancies, [15, 19, 20, 21]
5) Dynamic recovery, resulting from the relief of long-range back stresses due to vacancy formation and diffusion, leading to the climb of edge dislocations. [15, 19, 22, 23, 24]

In general the order given in this partial listing of thermally activated mechanisms is that of increasing activation energies. The last two processes involving the formation and migration of vacancies are the most difficult mechanisms having the highest activation energies. They can only take place at sufficiently high temperatures under conditions of slow deformation or creep, and therefore are not significant in the usual realm of dynamic conditions. But the remaining processes can be strain-rate controlling under certain appropriate dynamic conditions.

IV. THE INTERSECTION MODEL

In order to illustrate the origin of strain-rate sensitive deformation when thermally-activated mechanisms control, we will consider the case of the intersection mechanism. This choice is based on the fact that the intersection mechanism has been explored more thoroughly than any other mechanism and on the fact that there are good data available to correlate creep, slow tension, and dynamic test data. Due to limitations in time and

space, however, we will omit a detailed description of the factors involved here and refer the reader to some of the original discussions on this mechanism.

Seeger has shown that the plastic shear strain rate, $\dot{\gamma}$, for the intersection mechanism is given by

$$\dot{\gamma} = N A b \nu e^{-\frac{U}{kT}} \tag{4}$$

where

N = the number of points per unit volume of contact between the forest dislocations that are being cut and the glide dislocations

A = the average area swept out per intersection

b = the Burger's vector

ν ≃ the Debye frequency

k = the Boltzmann constant

T = the absolute temperature

U = the average energy that must be supplied by a thermal fluctuation to complete intersection.

When the dislocations are dissociated into Shockley partials, the average force, F_0 , versus displacement x , diagram at the absolute zero for intersection is given by the solid intersecting curve of Fig. 2.[25] As the dislocations are brought together x decreases and F_0 increases. As x decreases the dislocations first constrict, the constriction being completed at $x = b$. Between $x = b$ to $x = 0$ jogs are produced.

At temperatures above the absolute zero, the force is given by

$$F = F_0 \frac{G}{G_0} \tag{5}$$

where G and G_0 are the shear moduli at the test temperature and the absolute zero respectively. Eqn. 5 merely reflects the fact that both the constriction energy and the jog energy are linearly dependent on the shear modulus of elasticity.

If L is the mean spacing of the forest dislocations, a force

$$F = (\tau - \tau^*) Lb \tag{6}$$

will act at the point of intersection where τ is the externally applied shear stress, and τ^* is the back stress due to local interactions and long range stress fields. For our present objectives it will not be necessary to separate these two effects. If a force F less than F_m is applied, the energy that must be supplied by a thermal fluctuation in order to complete intersection is given by the modified Basinki equation

$$u = \int_{(\tau - \tau^*) Lb}^{F_m} x dF \tag{7}$$

Eqns. 4 to 7 plus the data of the $F_0 - x$ curve given in Fig. 2 then completely describe the intersection mechanism. It is significant that none of these equations contain explicitly the plastic shear strain, This arises because the shear strain itself is never a physically significant variable. Rather the physically important variables, at least for the intersection mechanisms, are the structurally significant quantities of $\tau^* = \tau_0^* \frac{G}{G_0}$, and L . In general these are not simply related to the strain but are also dependent on the temperature strain - rate history of straining. As deduced from slow tensile tests for $\dot{\gamma} = 10^{-4}$/sec. and $T = 90^{\circ}K$, they are given in Fig. 3.[25] When the metal is strained, L decreases and τ^* increases, both factors contributing to strain hardening.

V. SEEGER'S APPROXIMATION

It will prove instructive to first formulate the intersection mechanism in terms of Seeger's approximation which neglects the effects of constriction and is therefore appropriate for intersection of undissociated dislocations. Under this simplification

$$u = u_j - (\tau - \tau^*)Lb^2 = u_{jo}\frac{G}{G_o} - \left(\tau - \tau_o^*\frac{G}{G_o}\right)Lb^2 \tag{8}$$

This arises because the $F-x$ diagram no longer contains the curved part of Fig. 2 due to the constriction force and is then represented by a rectangular area from $0 \leq F \leq F_m$ and $0 \leq x \leq b$. Introducing Eqn. 8 into Eqn. 4 gives

$$\tau\frac{G}{G_o} = \tau_o^* + \frac{u_{jo}}{Lb^2} - \frac{kTG_o}{Lb^2G}\ln\frac{NAb\nu}{\dot{\gamma}} \quad \text{for } T < T_c \tag{9}$$

$$\tau\frac{G_o}{G} = \tau_o^* \quad \text{for } T > T_c \tag{10}$$

where

$$u_{jo} = kT_c\ln\frac{NAb\nu}{\dot{\gamma}} \tag{11}$$

The critical temperature, T_c, is that temperature at which the necessary thermal fluctuations needed to assist the applied stress in order to complete intersection occur practically instantaneously. Below that temperature, as shown by Eqn. 9, the flow stress decreases linearly with an increase in the absolute temperature. These trends are shown in Fig. 4 for the case of a mildly strain hardened metal where, as given in Fig. 3, 1/L was selected

to be $7.3\times10^{+5}$ 1/cm and τ_0^* was taken as 4.8×10^8 dynes/cm^2. For a given $\dot{\gamma}$, T_c remains insensitive to all accessible strain hardened states. Therefore $NAb\nu$ is substantially a constant. In order to complete the approximation $NAb\nu$ was estimated to be about 120 1/sec. and u_{jo} was approximated to be about 1.45×10^{-13} dyne-cm from Fig. 2 by neglecting the constriction energy. For $0 \leq T\frac{G_0}{G} \leq \tau_0^*$ the behavior is entirely elastic. And over the range $\tau_0^* \leq T\frac{G_0}{G} \leq \frac{u_{jo}}{Lb^2} + \tau_0^*$ plastic deformation occurs by thermally assisted intersection, the flow stress being dependent on the strain rate. As shown by Eqn. 9, the higher the test temperature, the more rapidly does the flow stress in this range increase with an increase in strain rate. Therefore tests conducted at low temperatures on only mildly strain hardened metals may have flow stresses that are substantially insensitive to the strain rate, whereas the same metal tested in a severely strain-hardened state and at higher temperatures may exhibit significantly sensitive variations of flow stress with strain rate.

An example of the effect of straining on increasing the strain-rate sensitivity of dynamically tested high purity Al is shown in Fig. 5.[26] The log $\dot{\epsilon}$ versus σ curves, which were obtained using a modification of Kolskey[27] thin wafer technique, are less steep for the material at higher strain hardened states over the lower ranges of $\dot{\epsilon}$ where $\ln\dot{\epsilon}$ is linear with σ, as dictated by Eqn. 9.

When $\tau\frac{G_0}{G} > \frac{u_{jo}}{Lb^2} + \tau_0^*$, thermal activation is no longer required to assist intersection because the force F at the point of intersection now exceeds F_m. Consequently the applied stress alone is more than sufficient to effect intersection. Obviously the intersection mechanism

is no longer rate controlling and some other dislocation mechanism now becomes significant in dictating the dynamic plastic behavior of the metal.

VI. PREDICTIONS OF DYNAMIC BEHAVIOR CONTROLLED BY INTERSECTION

In this section the dynamic behavior of Al will be predicted up to the limit of the intersection mechanism from the data recorded in Figs. 2 and 3 and the intersection theory given in Section 4. Since these predictions follow directly from the previous discussion the details need not be given here except to state that the assumption that the material obeyed a mechanical equation of state was made. The experimentally determined dynamic behavior and the predicted behavior are recorded in Fig. 6. As noted these predictions are excellent.

Above a strain rate of about $\dot{\epsilon} = 10^2$/sec. the applied force is sufficiently great to insure intersection without waiting for a thermal fluctuation and some other dislocation mechanism becomes rate controlling. This region has not yet been explored in sufficient detail to provide a clear picture of the various issues that might be involved. For example, deformation in this region could be due to the thermally aided production of vacancies as a result of the motion of jogged screw dislocations. It is a simple matter to show that for this mechanism also, $\ln \dot{\epsilon}$ would be proportional to σ, albeit with a different slope than that for the intersection mechanism. Since this prediction is inconsistent with the experimental facts shown in Fig. 6, it becomes obvious that the high strain-rate effects shown in Fig. 6 cannot be ascribed exclusively to the thermally activated motion of jogged screw dislocation. It is of course possible that a number of different difficult dislocation mechanisms contribute to

the high strain-rate behavior. A short time ago the authors[26] suggested that the leveling off of the strain rate might be due to the fact that the dislocations were reaching their limiting velocity, c, namely the velocity of a shear wave. At this limiting velocity, the limiting strain rate $\dot{\gamma}_e$ is

$$\dot{\gamma}_e = Mbc \tag{12}$$

where M is the density of dislocations. For the highest experimentally observed strain rates, however, M must be taken to be at least two orders of magnitude less than the actual dislocation density in order to achieve agreement with the experiment. Obviously the assumption of relativistic velocities of dislocations in this case is highly untenable.

When the data given in Fig. 6 are replotted on linear scales, the results shown in Fig. 7 are obtained. Here it is clearly evident that a limiting relativisticly controlled strain rate is not being approached. Rather the strain rate appears to be a linear function of the applied stress. The linear dependence of the strain rate on the stress must arise from the operation of some yet unidentified dislocation velocity-sensitive dissipative force.

It is expected that at sufficiently high values of stress, disloca-tions might acquire the velocity of sound as originally predicted by Frank.[28] An example of an approach to this condition is given by the data of Johnston and Gilman[29] who measured the velocities of individual dislocations as a function of the applied stress. Their data, reproduced in Fig. 8, clearly illustrate that the dislocation velocity at high stresses approaches asymptotically that for a shear wave.

VII. DYNAMIC SUPPRESSION OF HIGH TEMPERATURE THERMALLY ACTIVATED MECHANISMS OF DEFORMATION

Slow mechanisms such as those dependent on diffusion cannot contribute to the dynamic straining. In this section a simple example of the suppression of such a mechanism will be described.

The critical resolved shear yield strength for prismatic slip of single crystals of the hexagonal phase of an alloy containing 67 atomic percent Ag and 33 atomic percent Al for shear strain rates of $\dot{\gamma} = 10^{-4}$/sec. are shown by the solid line of Fig. 9.[30, 31] Whereas Regions I and III represent deformation by thermally activated mechanisms, Region II, where the yield strength is insensitive to the temperature, is athermal. Since dislocations on the prismatic plane are undissociated, this athermal behavior cannot be ascribed to Suzuki locking[32] and is therefore believed, at present to result from short-range order hardening. The thermally activated mechanism responsible for plastic deformation in Region I cannot be intersection and is most likely the Peierl's mechanism since the activation volume is only about $v = 13\, b^3$. Phenomenologically, at least, it can be represented by the relationship

$$\dot{\gamma}_{I} = K e^{-\left\{\frac{U_p - v(\tau - \tau^*)}{kT}\right\}} \tag{13}$$

which is analogous to Eqn. 9 for intersection. Here, however, the strain hardening is negligible, v has the constant value of about $13\, b^3$ and τ^* refers to the stress necessary to disorder the alloy across the slip plane. Rather complete investigations[33, 34] conducted over Region III have shown that

$$\dot{\gamma}_{III} = 1.4\, \tau^{3.6}\, e^{-\frac{32,500}{RT}} \tag{14}$$

revealing that this is also a thermally activated mechanism, although it has not yet been definitely identified in detail. The major issue to be made here is that the yield strength for dynamic test conditions, for $\dot{\gamma}$ between 55,000 and 71,000 per sec. is given by the broken line. Thus for these dynamic conditions the Mechanism III operative over the high temperature region for slow strain rates appears to be supplanted by the Mechanism I appropriate only to the low temperature region when the strain rate is low.

VIII. STRAIN-RATE INSENSITIVE DEFORMATION

Over ranges where the flow stress is insensitive to the temperature, the flow stress is also insensitive to the strain rate. For intersection this occurs above the critical temperature T_c, as shown schematically in Fig. 4. It should apply also to Region II for prismatic slip of the Ag-Al intermediate phase, as shown in Fig. 9. Perhaps the most striking confirmation of this deduction is contained in the example of basal slip on the 67-33 atomic percent Ag-Al intermediate phase.

As shown in Fig. 10, the critical resolved shear stress for yielding by basal slip at slow shear strain rates of $\dot{\gamma} \approx 10^{-4}$/sec. is independent of the temperature from about 77° to 450°K. Above 450°K, however, the single crystals oriented for slip on the basal planes undertook instead, slip on the prismatic plane giving results in complete agreement with those recorded for Region III in Fig. 9. The slow strain rate tests revealed a yield point phenomenon with a tremendous yield strain of 135%. The presence of a yield point demands that the dislocations on the basal plane were locked whereas those on the prismatic plane were not locked. Since the yield point was insensitive to the temperature and since the atomic

radii of Ag and Al are almost identical, the observed locking could not be ascribed to Cottrell interactions. The only known mechanism that could simultaneously be responsible for the locking and the athermal behavior is the Suzuki mechanism. Such locking could only occur on the basal plane where dislocations dissociate into their partials to provide the stacking fault necessary for chemical locking.

The dynamic yield strength obtained for strain rates up to 6×10^3 per second are shown by the solid curve of Fig. 10. Over the high temperature range, the thermally activated prismatic slip noted in slow strain-rate tests was suppressed and only basal slip was observed. Here, as is only possible for Suzuki locking, the critical resolved shear stress increased somewhat as the temperature increased due undoubtedly to more complete chemical locking of the dislocations. And over the low temperature range from $77^{\circ}K$ to $450^{\circ}K$, the flow stress obtained in the dynamic tests were only slightly higher than those obtained for slow rates of deformation. This case then represents an extreme example of strain-rate insensitive deformation.

ACKNOWLEDGEMENTS

This paper was written under the auspices of the Inorganic Materials Division of the Lawrence Radiation Laboratory. The authors wish to express their appreciation to the United States Atomic Energy Commission for support and sponsorship of the research. They also wish to thank Mr. S. Rajnak, Mr. J. Mote and numerous graduate students for their cooperation on the experimental phases of the investigations described here.

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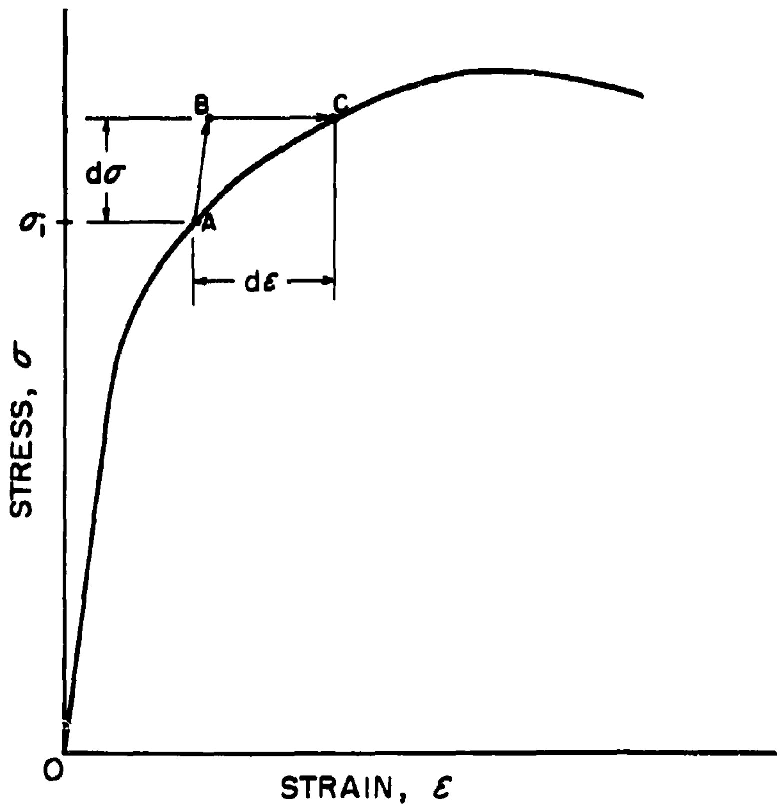

FIG. 1 STRESS - STRAIN CURVE FOR INFINITELY SLOW DEFORMATION RATES.

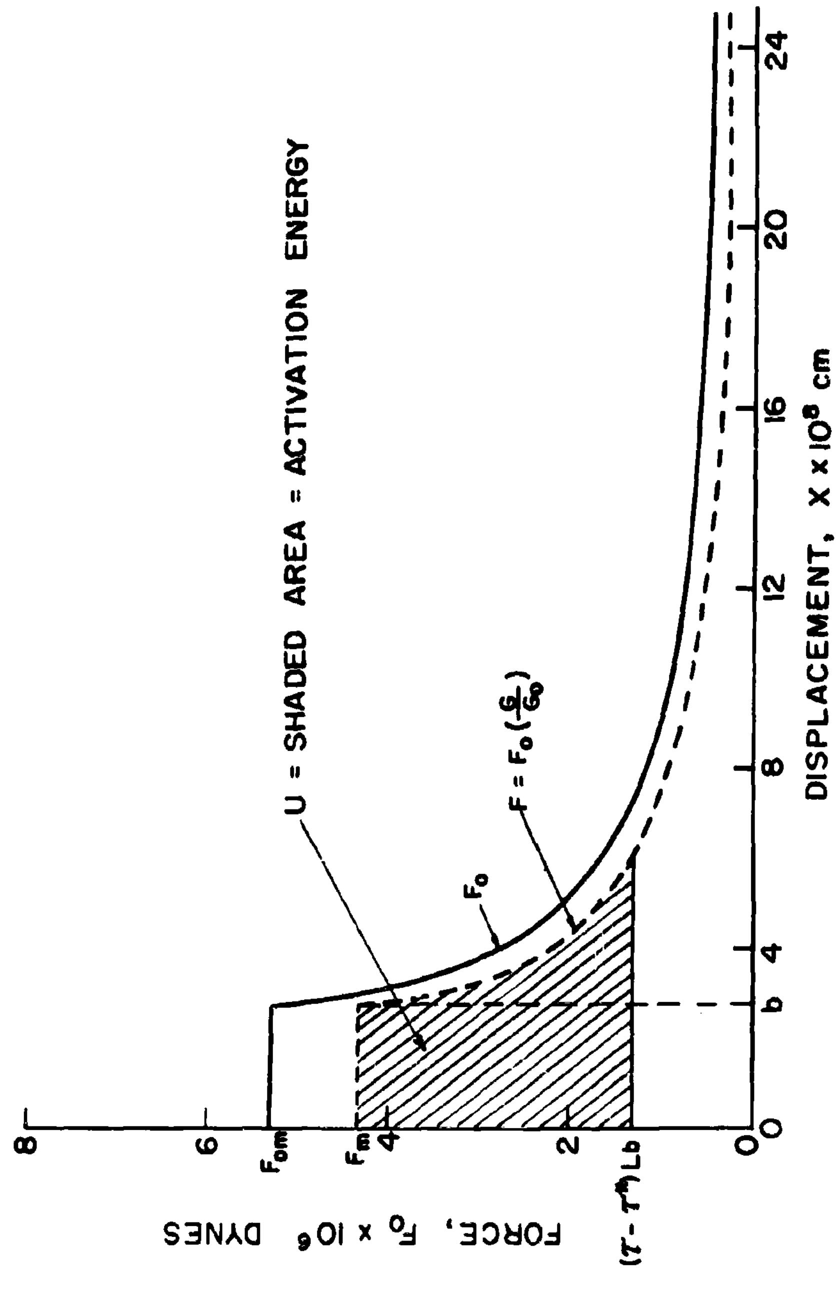

FIG. 2 FORCE - DISPLACEMENT DIAGRAM FOR INTERSECTION IN PURE Al (25).

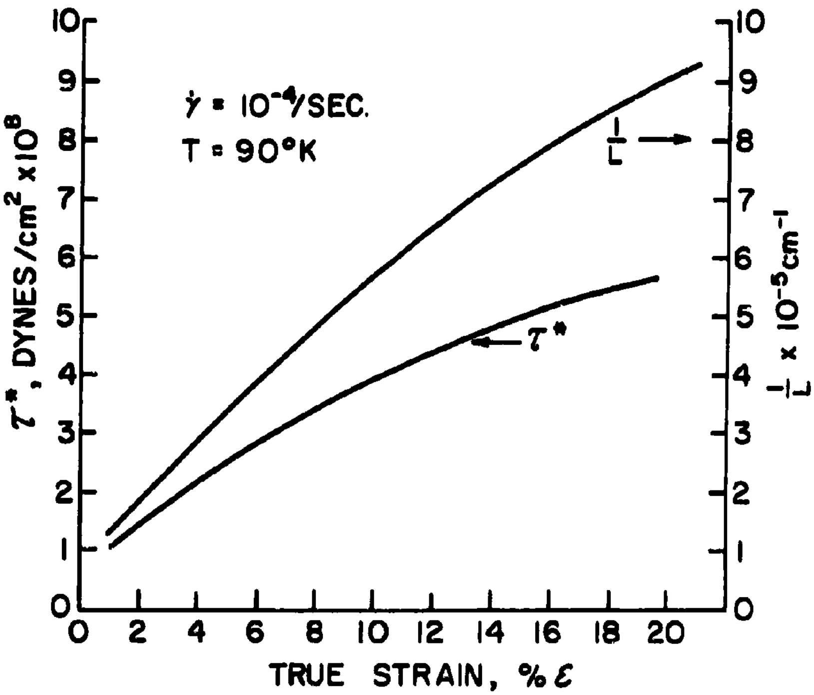

FIG. 3 VARIATION OF DISLOCATION SPACING AND BACK STRESS FIELDS WITH STRAIN IN PURE Al (25).

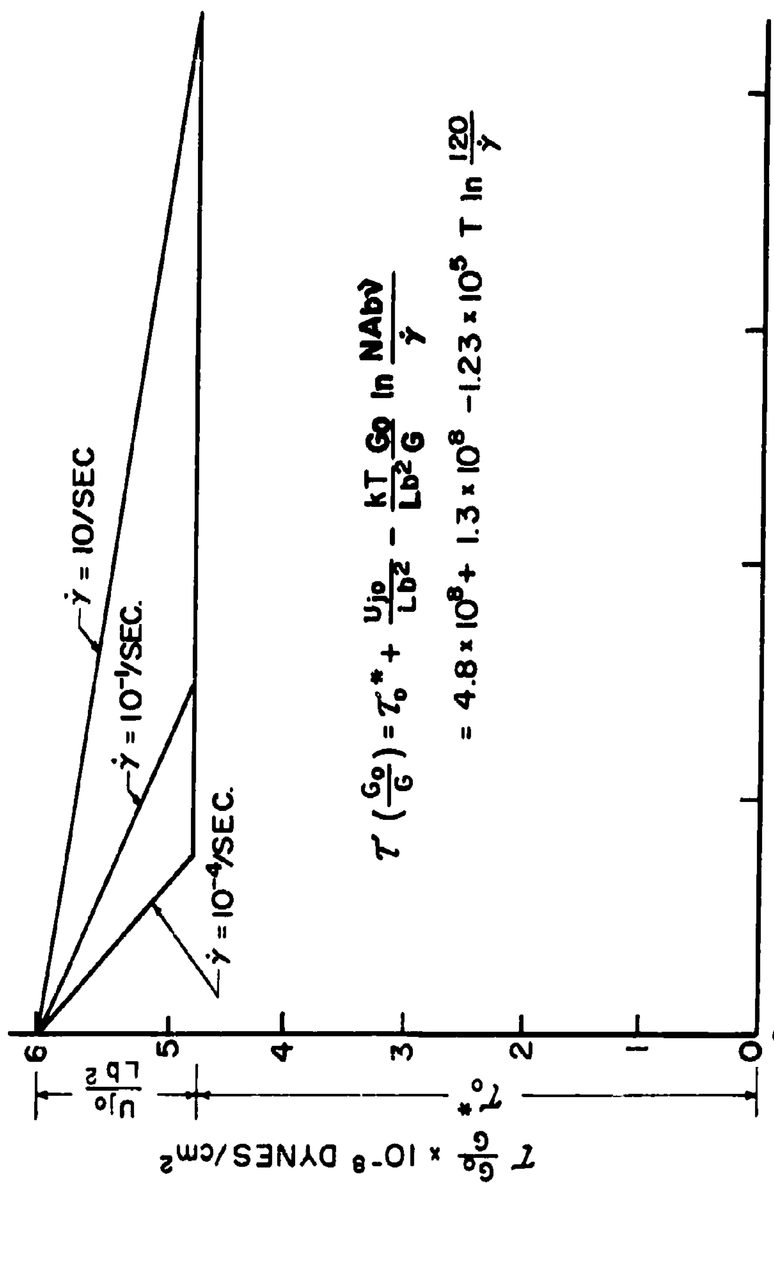

FIG. 4 REPRESENTATION OF THE STRESS-TEMPERATURE RELATION FOR INTERSECTION AT A GIVEN STRUCTURE.

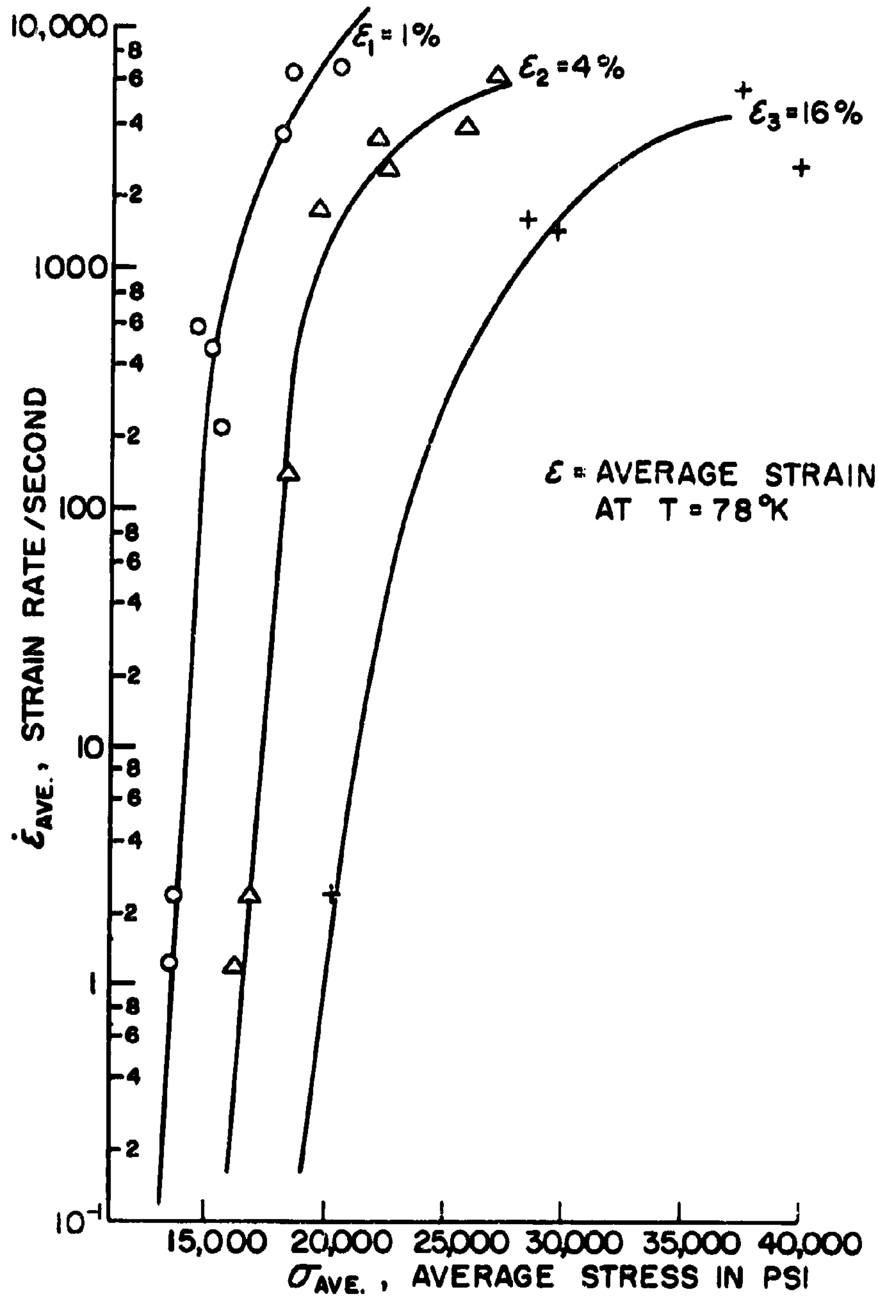

FIG. 5 EFFECT OF STRESS ON STRAIN RATE AT CONSTANT STRAIN (26).

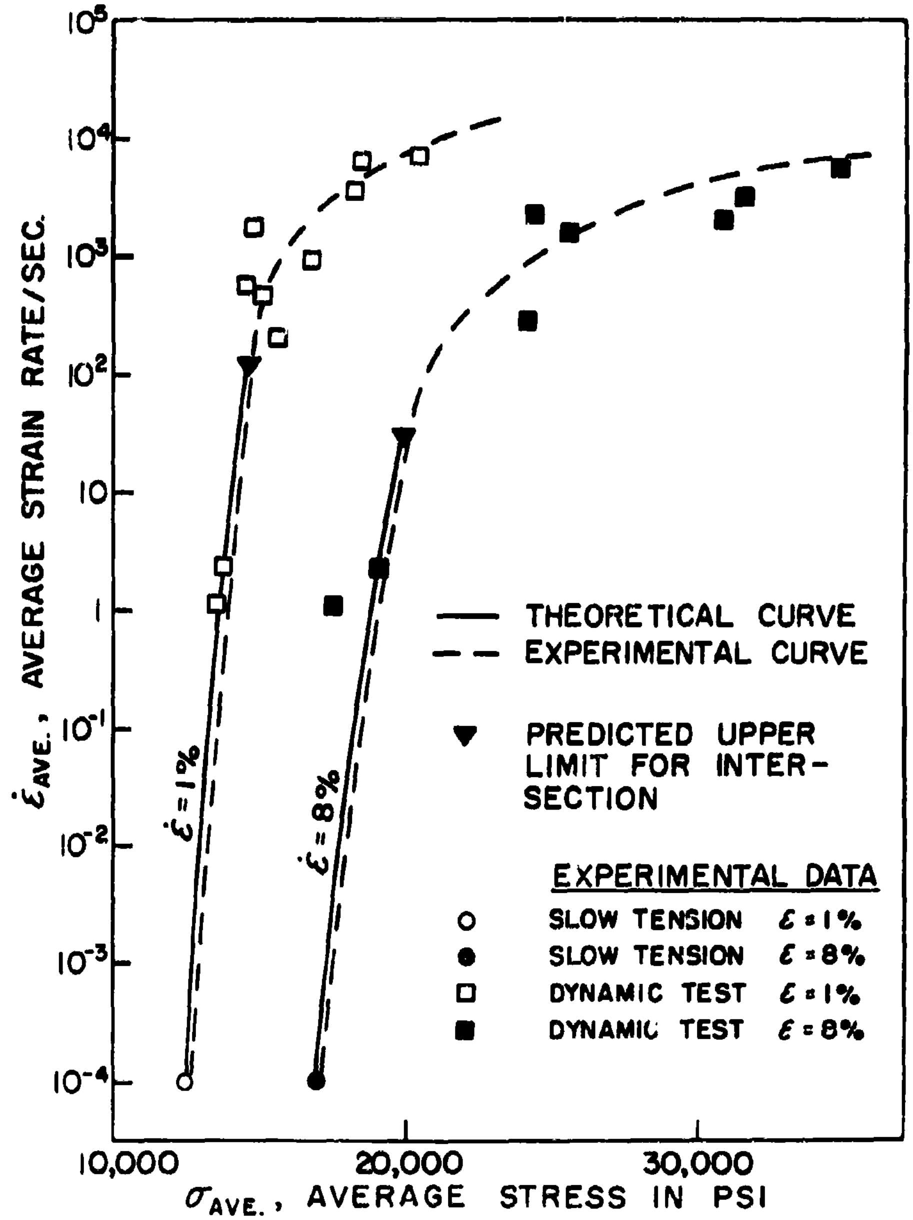

FIG. 6 PREDICTION OF THE DYNAMIC PLASTIC BEHAVIOR IN THE INTERSECTION REGION FOR PURE Al AT T = 77°K.

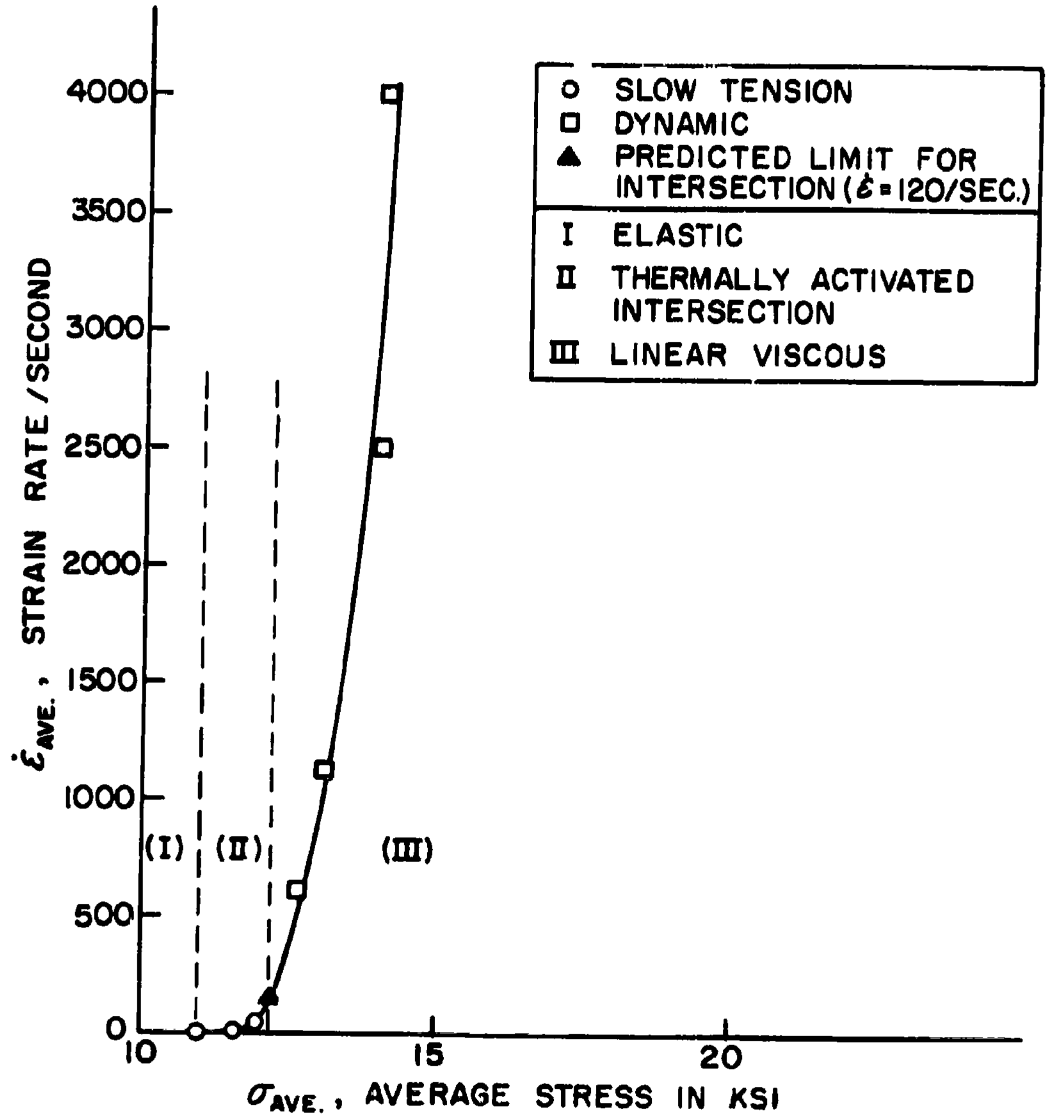

FIG. 7 EFFECT OF STRESS ON THE STRAIN RATE IN DYNAMIC TESTS IN PURE Al AT 295°K.

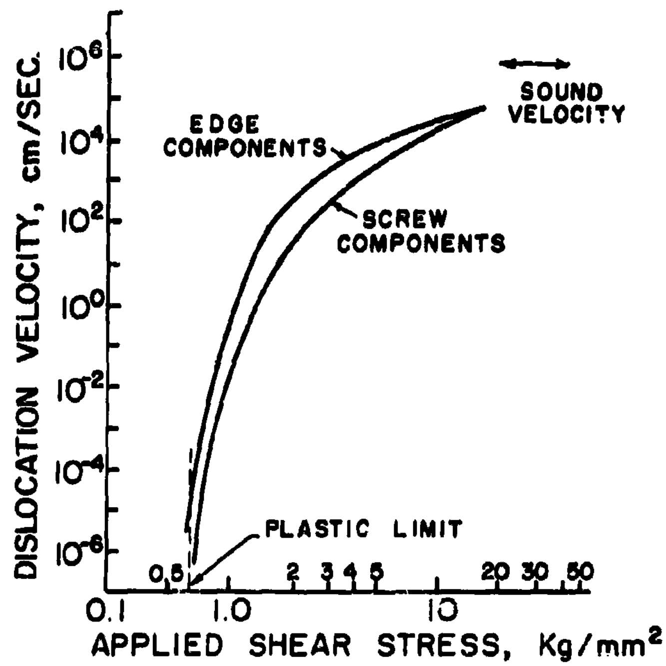

FIG. 8 DISLOCATION VELOCITIES IN LiF AS A FUNCTION OF STRESS (29).

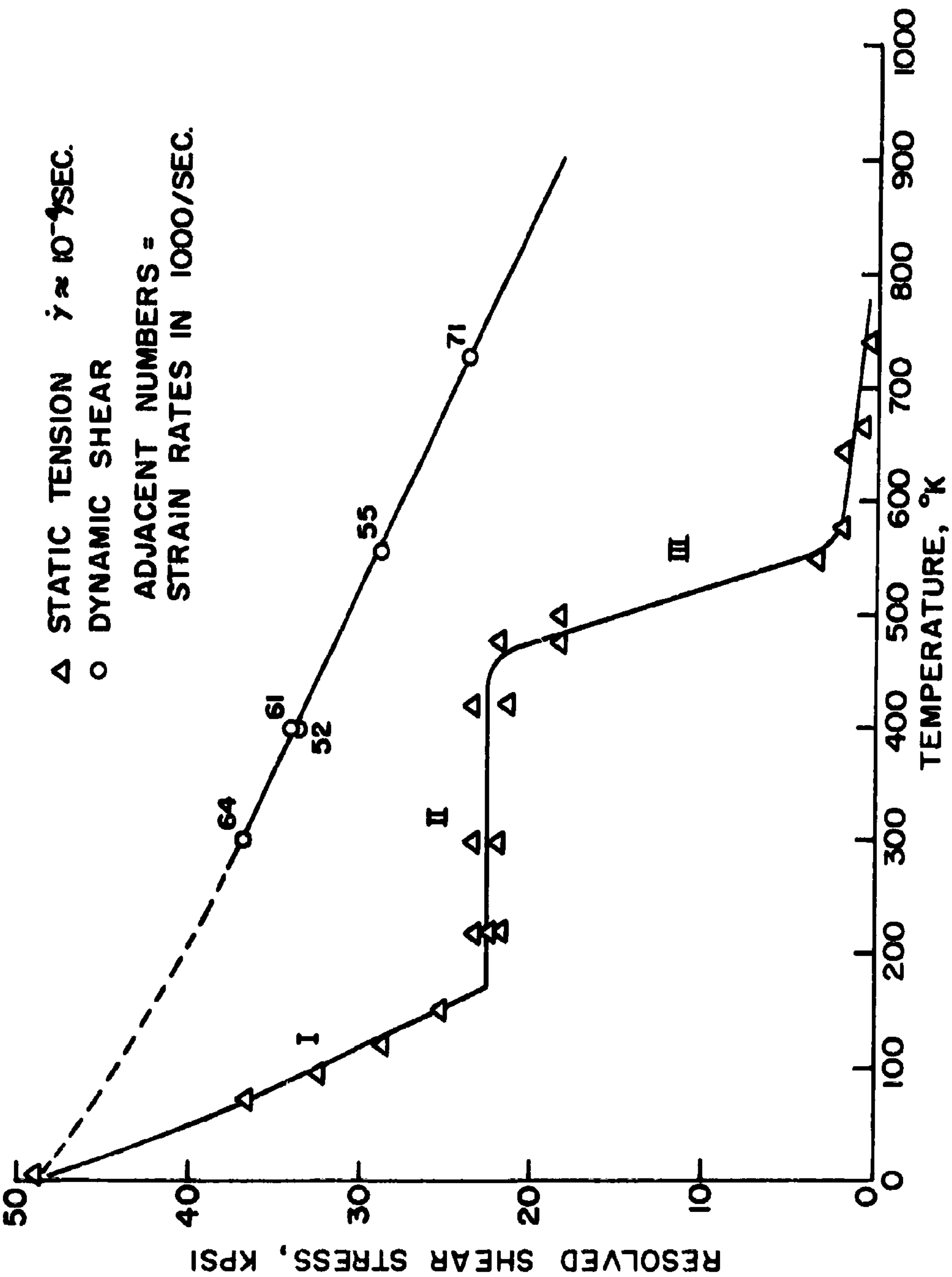

FIG. 9 PRISMATIC YIELDING OF AN Ag (67%)-Al (33%) ALLOY (30, 31).

DISCUSSION

DR. GOLDSMITH

Professor Dorn's paper is now open for discussion.

FROM THE FLOOR

Say you have a plastic wave and then what might be a good step in plastic waves, how does the plastic wave velocity relate to this dislocation velocity that you are talking about? Is it a motion of propagation in a kind of new medium? How does it act?

DR. DORN

Well, I think most people are very familiar with the von Karman-Taylor type of approach, and in this type of approach we are using the Lagrangian Coordinate time plane. What happens is, you impact the bar. The leading characteristic (these are all hyperbolic equations) has a velocity of sound in the material and then there are a series of other characteristics that come down here (pointing to slide). This stress corresponds to the stress that was generated under impact, and you can see exactly what is happening to the waves here as a function of spreading out as time increases. The mutualistic theory has been completely described in terms of the Malburn concept, and all of the analytical tools are available to apply, well, to very simple cases. To apply to any simple case of say linear stress work down the bar and in this, again, the situation is a series of characteristics of this time, plus, characteristics that lie along here; and the integration must be done along these characteristics. The impact is down at the high stress level and this very high stress flows along here, but as plastic deformation takes place, the stress level decreases all the way, you see, as it happens and then it is a telegraphic sort of thing. There is a signal brought over here and a signal brought over here, and step by step integration can be used; it can't be done in any other way. Usually the equations are so complicated, that an analytical solution is not possible. So if we have a strain-rate sensitive material, then this type of approach that was first suggested by Malburn is an appropriate approach.

DR. GOLDSMITH

Are there any other questions?

ANDERSON, AEROJET

Could you comment briefly about absolute reaction rate theory to describe flow involving dislocations?

DR. DORN

The absolute reactionary theory is only one of the approaches to the thermally activated processes. As a matter of fact, it is identical with what I suggested here relative to the intersection mechanism. Care must be exercised not to think that the stress always comes

directly from the activation energy term linearly, and it may be very complicated in dislocation mechanisms. The mechanical energy may be frequently a very complicated function of the stress. But with this exception, it is identical with what I did here.

FROM THE FLOOR

I am interested in something which is probably an unimportant point in the general discussion of the Basal Slip with the yield strength going up with temperature. This is of great interest to me because of the general questions involving heat that we talked about before. I wonder if you have any general information about whether the A Thermal variations will always show an increase of yield strength with temperature?

DR. DORN

The only example I know of where an increase in flow stress with temperature has been seen is the one that I quoted here. In this particular case, although I'm not certain about this, I have the impression that this is the result of a change in the distribution coefficient between the stacking salt and the crystal proper as the temperature increases. This brings about an increase in energy necessary to break the two partial dislocations away from this difference in composition. I can't say anything about the generality of this, as I would prefer. In the case of other A Thermal processes, for example, in the case of merely overcoming long range back stresses, the flow stress will decrease linearly with temperature in the same way as the sheer modulus decreases with temperature. You will not have this increase that was shown here because the flow stress, the long-range back stresses, resulting from dislocation are linearly related to the sheer modulus, and the sheer modulus decreases with temperature, and so we have a little decrease in the flow stress with temperature in the A Thermal processes.

FROM THE FLOOR

Going back to the beginning of your lecture, you said something about time scales and how much energy is available from the stress wave to decide whether it is thermally active or inactive. I wonder if the time scales are such that you can now explain their existence with this model of incremental waves propagating incremental elastic waves in an elastic prestressed field. We have observed these incremental waves in the annealed aluminum at high strain in their gross behavior.

DR. DORN

I don't know whether I can completely explain that. I've thought about this. I think the technique that you are using is very good. I attempted to illustrate, however, in talking about the intersection mechanism, that if you were to use an annealed material, you must necessarily have a very small strain rate effect, but not a zero strain rate effect. First, you have a large strain rate effect, then if you have an overstress above this value, a very small strain rate effect will follow because of the dependence of the flow stress on the strain rate through the density of the dislocations.

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FROM THE FLOOR

I was thinking of propagating a very small lattice wave through the medium where you have an amplitude stress.

DR. DORN

Oh, that has been done by a number of individuals as you well know. Curtis did this and one other gentleman did this and in all cases the wave traveled not with the von Karman velocity, but with the true velocity of sound, as you would have expected in terms of the Malburn approach.

FROM THE FLOOR

My point was, does it in the annealed aluminum that I was describing also? That is if you can prestress it say with impact, then reflect a small elastic wave back through, you will observe that it goes with the von Karman velocity. Now you said something about the stress level--in a certain sense the material is going to be elastic.

DR. GOLDSMITH

In the interest of conserving time, we will have to close this discussion. I would like to again thank Professor Dorn for a very interesting presentation.

The last paper in this afternoon session is concerned with the first two words of the topic of the meeting "Structural Dynamics" and I am particularly anxious to give Professor Hoppmann all the room or time as the case may be that he can use to expound his subject. Dr. Hoppmann will speak on "Some Problems in Dynamics Response of Two Dimensional Structures." The speaker received his Ph.D. degree from Columbia University. He spent a considerable amount of time at Johns Hopkins University as a Professor there and has recently moved to Rensselaer. He is the author of a number of papers in the general area of structures, impact and wave propagation, and particularly we know him for his work on orthotropic plates. Professor Hoppmann.

PROFESSOR HOPPMANN

Thank you Dr. Goldsmith. To conserve time, and with your kind permission, I will read my paper.

SOME PROBLEMS IN DYNAMIC RESPONSE OF TWO-DIMENSIONAL STRUCTURES

by

W. H. Hoppmann II, Ph.D.

Rensselaer Polytechnic Institute

SOME PROBLEMS IN DYNAMIC RESPONSE OF TWO-DIMENSIONAL STRUCTURES

W. H. Hoppmann II

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ABSTRACT

The paper presents a discussion of the problem of dynamical response of simple two-dimensional structures such as plates and shells to impulse loading. Consideration is given to the relationship between propagation of waves in the medium and the normal mode vibrations of the medium. Consideration is also given to the connection between the theory of dynamics of the three-dimensional continuum and that for the two-dimensional structures. The need for studies on one-dimensional bars is taken into account. The rôle of the constitutive equations and change of state of the material under load is briefly discussed. Finally motivations for various kinds of studies on the subject and also future needs are surveyed.

SOME PROBLEMS IN DYNAMIC RESPONSE OF TWO-DIMENSIONAL STRUCTURES

INTRODUCTION

The stated purpose of the symposium is to establish a complete coordinated program for study of the dynamics of solids subjected to impulse loads. As a preliminary step to attaining such an objective, a request has been made for a statement on the present state of knowledge of each important aspect of the problem and then for an indication of where efforts should probably be exerted in the years immediately ahead. Undoubtedly a program of such scope is ambitious and fraught with difficulties. Its success demands the efforts of scientists who naturally do not respond sympathetically to sharply defined directives for specific plans of research. Such a situation is psychologically delicate and requires that everyone concerned proceed with prudence in order to attain progress in the area outlined. Certainly a scientist should never be put in the position of constraining himself to follow arbitrary directives in order to procure contracts whereby he may be able to staff his laboratory and increase the number of graduate students working on his research projects. It does seem plausible however that any scientist should from time-to-time survey the field of his particular interests and try to make some assessment of what has been accomplished, what is being accomplished, and what probably will be accomplished. Surely real progress depends on such taking of stock. It is in such a vein that the present paper is presented. With proper respect for the many accomplishments of others, the author addresses himself to the task of trying to sketch intelligibly some of the problems in structural dynamics which have been and will be of concern to himself. As is natural his work reflects that of many other researchers in the field.

The symposium is devoted to a subject with many facets. The present paper is mainly limited to that aspect which is primarily concerned with the response of two-dimensional structures subjected to surface loads. Of considerable importance is the relationship of travelling

strain waves and the overall dynamic response of structures. Although the primary concern in this paper is with two-dimensional structures, some consideration will be given to the dynamics of the one-dimensional continuum.

STRAIN WAVE TRANSMISSION AND NORMAL MODE VIBRATIONS

In any practical study of the dynamic response of structures to impulse loads, it becomes necessary to determine the geometric configuration of the strained structure at any instant and hence to a reasonable degree of accuracy its strain distribution. It should be clear that if a two-dimensional structure, such as a plate or shell, is subjected to an impulse load on its surface, a strain wave will be set in motion and will be reflected in a complicated manner from the surfaces. The strain state at any time is composed of the effects of the travelling waves. To the writer's knowledge, no investigator has as yet traced the strain waves generated by the surface loads on finite two-dimensional bodies and thereby deduced the total strain state at any instant. Perhaps the tacit assumption is usually made that, in principle at least, the analysis is possible but extremely complicated. One may reasonably inquire whether in terms of present day mathematical development this is even possible. No doubt considerable study of this phase of the problem is needed. However, in the meanwhile can one obtain some useful knowledge of the deformed state of a structure from what may be called a normal mode analysis based on stress-resultant theory? The answer to the question is certainly affirmative, at least to a degree. The success of the Bernoulli-Euler theory of the beam and the analogous Lagrange-Germain theory of the plate give ample evidence. Nowadays one is quite well aware of the shortcomings of these theories but no one can deny that they serve as a substantial starting point in any study of the dynamic load problem. In fact they constitute the first step in the generalization of the successful mechanics of the statics of structures to the mechanics of structures subjected to time dependent loads. Significant improvements have been made in these theories by consideration of shear deformation and rotatory inertia. One generalization of the Lagrange-Germain plate equations allowing for shear deformation and rotatory inertia has been deduced from the three-dimensional theory of small elastic strain by R. D. Mindlin [1]. It is interesting and

instructive to note that Bernoulli and Euler derived the elementary beam flexure equation before the advent of the equations of elasticity. This classic example in the history of mechanics throws revealing light on the different ways in which one might profitably approach the study of any problem in mathematical physics. An example of the fruitful combination of stress-resultant theory and the three-dimensional theory of elasticity to solve problems in the dynamics of structures is demonstrated in a paper on the vibration of thick walled cylinders by J. E. Greenspon [2]. Also, a fairly long treatment of both waves and normal mode vibrations in isotropic plates is given in a very useful paper by R. D. Mindlin [3]. In this latter paper however the study is limited to cases of plane strain in either infinite plates or in plates with only straight boundaries. The methods used should be of some value in attempts to solve problems for plates with curvilinear boundaries. Also it may be seen that great care was exercised in assessing the various aspects of the problem. Finally it may be observed that a pertinent bibliography was appended.

The literature on strain wave transmission and normal mode vibrations in solids up to 1953 is discussed in a review article by R. M. Davies [4]. Also, a somewhat more extensive review of the Russian literature on the subject is given in a chapter on the dynamics of structures contained in a report on structural mechanics in U.S.S.R. from 1917 to 1957 [5].

Application of these various theories to design problems in structural engineering is further complicated by the need for stiffeners in various structures. Some thought will now be given to this important problem.

ANISOTROPY AND STIFFENED STRUCTURES

If strain wave analysis is complicated in the case of isotropic plates and shells it is even more so if we deal with anisotropic materials and stiffened structures. Anisotropic plates usually occur as natural crystals, isotropic material with external stiffeners attached, or materials such as polymers impregnated with metallic wires. Other examples occur in applications of reinforced concrete.

Anisotropy adds to the difficulties of analyzing strain wave transmission in solids, because the number of elastic compliances for the anisotropic substance increases above those for an isotropic material. Accordingly, in such cases, different wave velocities exist for different directions. In order to determine anisotropic elastic constants, suitable experiments must be conducted and of course the useful interpretation of the experimental results will depend upon the development of appropriate theory. An excellent monograph has been written on these problems of anisotropy for plates by R. F. S. Hearmon [6]. In his small book Hearmon presents clearly an introduction to the theory of anisotropic elastic substances and also includes some reference to experimental investigations as well as to approximate theories. Here again we see the value of normal mode and stress-resultant theory. The literature survey reflected by the work is up-to-date and very useful. Anyone concerned with the problems of anisotropic structures would do well to study the material developed there.

An important analogue to the pure crystalline plates is supplied by stiffened two-dimensional structures. Ever since Huber in 1914 [7] began the study of the statical bending of a stiffened plate as an analogue of the flexure of a uniformly thick orthotropic plate, the literature of this subject has been rapidly increasing. The present writer in the early fifties tried to generalize the work of Huber and make it more available to the structural engineer. The first results of these investigations were published in a paper in 1955 [8]. Since that time a considerable advance has been made in solving some of the problems. The power of the methods can be seen further developed in the analysis of the moderately large deflections of stiffened rectangular plates with arbitrary elastic boundary restraints in a doctoral dissertation by W. G. Soper [9]. The methods used by Soper should be generalized for the case of dynamic loads.

If stiffeners are attached to a plate or shell it is obvious that strain wave analysis becomes prohibitively difficult. Reflections of the strain waves from individual stiffeners must take place thus producing an amazingly complex pattern. Nevertheless, normal mode analysis for plates and shells with stiffeners can be made to yield very useful results.

In the case of stiffened two-dimensional structures, the rotatory inertia becomes of much more significance than for plates and shells of uniform thickness. A convincing illustration of the importance of this fact is given in a rather recent paper by Thorkildsen and Hoppmann [10]. In the paper comparison is made between experimental and theoretical results for vibrating aluminum plates with integral stiffeners.

Nodal pattern studies for vibrating plates with stiffeners are very interesting and important to the understanding of the dynamics of such structures. Some experimental results which illustrate the phenomena are presented in a paper by Hoppmann and Magness [11]. As learned by experience, experimental studies of frequencies of vibration should always be accompanied by determinations of the nodal patterns. Methods for calculating the frequencies of vibration and determining the normal mode shapes of orthotropic rectangular plates with various boundary conditions are illustrated in a paper by Huffington and Hoppmann [12]. That the theory of vibration of plates with stiffeners can be generalized to include stiffened shells has been demonstrated in several papers by Hoppmann [13,14,15]. In one of these papers certain important relationships existing between frequencies and modal shapes for the vibration of circular isotropic cylinders, discovered by Arnold and Warburton, are shown to exist also for the orthotropic circular cylinder [13].

Both for studies of plates and shells, the paucity of results in the literature of special functions and their defining differential equations hinders progress. An example of this difficulty is illustrated in a paper on stiffened plates by Hoppmann [16] and in a recent paper on stiffened shallow spherical shells by Hoppmann and Miller [17].

Some of the practical information which can be obtained from studies of two-dimensional stiffened structures as analogues of uniformly thick anisotropic structures include knowledge of displacements, frequencies of vibration, nodal patterns, and buckling loads. However, it is clear that the precise determination of strain at a point of concentration near a stiffener cannot yield to the proposed type of analysis. A specific study of stress patterns in a vibrating plate having stiffeners is given in a paper by

Hoppmann, Huffington, and Magness [18].

If yielding and plastic flow result from the dynamic loads then additional difficulties will arise in analyzing the response of structures. These phenomena have not as yet been studied from the standpoint of suitable theory and related experiments. Of course, even for so-called isotropic substances dynamic phenomena have not been extensively studied. On the other hand, a great deal of study has been devoted to the dynamics of the one-dimensional bar in which yielding and plastic flow have occurred. It may be useful here to recall a few of the studies of the one-dimensional bar.

DYNAMICS OF THE ONE-DIMENSIONAL CONTINUUM

For many years now studies have been devoted to wave analysis of the one-dimensional continuum. The classical examples are the string and the bar. A considerable portion of the literature on the subject has been cited in the previously mentioned paper by Davies [4]. It may be appropriate to add here the reference to a few of the studies not mentioned by Davies and also, a few of the accomplishments reported since that time. For example, Mindlin and Fox have presented an exact solution for the problem of strain waves in a bar of rectangular cross-section [19]. A fairly long study of various problems of impact on bars has been presented in a very useful small book by W. Goldsmith [20].

Considerable study has been devoted to the problem of transverse dynamic loading of bars both from the standpoint of the Bernoulli-Euler theory and the various improvements, known under the heading of Timoshenko beam theory in which allowance is made for rotatory inertia and shear deformation. It has been demonstrated that useful knowledge of beam response to transverse impact loads can be deduced from the elementary theory. Specifically it was discovered by Hoppmann that the initial response of a beam to a moving mass which collides with it is very well predicted by the simple theory [21]. These results were amply verified in a beautiful experimental study made subsequently in Germany by Emschermann and Rühl [22]. Some of the considerations arising in connection with allowance for shear and rotatory inertia are given in a very enlightening paper by Goland,

Wickersham, and Dengler [23].

Longitudinal and transverse impact on bars have been used for almost a century by metallurgists to provide knowledge on the ductility or brittleness of metals. Obviously, in these cases one must consider problems in plasticity. Here radical changes of state of the materials come into play. A survey of the general field is provided in the Proceedings of a Symposium edited by E. H. Lee and P. S. Symonds [24].

Various uses can be made of impact experiments with the one-dimensional continuum. An example is found in the knowledge gained about energy absorption in a bar as a function of velocity of impact. An extensive study of this phenomenon was made and results given in a 1947 paper by Hoppmann [25]. In this paper a relationship was pointed out between a so-called "critical velocity" which was observed in the experiments and the meaning of the now well-known von Kármán analysis for plastic waves in a bar subjected to longitudinal impact.

Longitudinal impact loading of a bar of circular cross-section was used by J. F. Bell to develop the ruled grating type of strain gage [26]. Recently studies were made by J. Sperrazza on the propagation of large amplitude waves in bars subjected to longitudinal impact [27]. The experimental techniques described in this paper are very interesting. For example a description of how the lead had to be prepared in order that the ruled gratings for measuring strain might be machined onto the specimens.

Many more examples of uses of the bar or one-dimensional continuum for studying response to dynamic loads might be added. The literature is rich in provocative findings which no doubt should stimulate other important research. In fact the bar experiments are one of the few means available at present for studying the various important dynamic properties of materials.

PROGRAMS AND GUIDE LINES FOR FUTURE RESEARCH

The warning sounded in the introduction of the present paper about the presumption involved in any attempt to lay down a research program for the future are not forgotten.

However, it is surely an individual's right to state what he personally thinks should be studied in the immediate future. It is in this context that the following brief suggestions are made. It is perfectly well appreciated that there are many other very important additional subjects and problems which may also be profitably considered.

For the purpose of effectively studying structural vibrations, the theory of special functions needs further development. At various times in the past the comment has been made that solutions of problems defined by differential equations can be obtained in terms of infinite series for which one need only determine the coefficients. No doubt certain merit attaches to such a viewpoint, however it is considered that this is not always the most enlightened or satisfactory approach. Functions with catalogued properties and tabulated values are very much needed. The need along this line was pertinently illustrated in several previously mentioned papers [16,17].

For use in analysis of vibrations of shells, the theory of factorization of differential operators for ordinary differential equations of higher order with variable coefficients definitely needs development. It is considered that the explicit factors will lead to solutions more readily and also shed more light on the analysis [17]. Some attempts at the development of the theory have been made by Ince and others but not nearly enough has been done.

Some complete solutions of the Love type equations for the flexural vibrations of shells of simple shape are sorely needed, particularly as guides to further development of the general theory. To the writer's knowledge no complete solutions of this nature are now in existence. Many studies of various kinds have been on the analysis of the dynamics of shells but when the results are carefully analyzed it is found that they do not constitute answers to the present question.

Normal mode shapes and their associated frequencies should be experimentally determined with great care for the simpler type shells. It should be emphasized that the frequencies should not be catalogued independently of the associated modal shapes. It is considered that a program of this type is very tedious but necessary for building up proper concepts of the nature of shell vibrations. Every

effort should be exerted to develop the appropriate analytical description of such vibrations.

Investigations should be made of the phenomena associated with large deformation of metals produced by high impulse loading. In particular the thermal characteristics of the changes of state involved should be intensively studied. Such a program is important for increasing our knowledge of the solid state but also it will aid in the design to structures which can most efficiently absorb given quantities of energy without complete collapse. Although this point-of-view would appear to be of more immediate concern to military pre-occupations, it is considered that it is of equal value in connection with the design of civil structures. In the study of specific energy absorptive capacities of materials, sight should not be lost of the equally important problem of designing mechanisms and composite structures to increase the ability to handle the incoming energy without serious deformation or at least without complete collapse.

Finally it is suggested that the investigation of wave transmission, both elastic and plastic, in bars be extended to include studies of wave transmission and flow in plates, shells, and three-dimensional solids subjected to high impulse loads.

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DISCUSSION

DR. GOLDSMITH

I would like to thank Dr. Hoppmann for a most interesting discussion. I might add two comments. First, it seems to me that . . . (inaudible) . . . in a talk I recently had the privilege of hearing by Professor Drucker titled "The Roll of Experiment in the Development of Theory." The second comment I would like to make is that from my own personal experience in making calculations of the kind that Professor Hoppmann talked about, I found on occasions that first order theory did not agree as well as the zero order theory in particular with respect to the new order of being. As a typical example of this I might mention that if you take your straight longitudinal wave propagation equation, and add to it under certain circumstances a lateral inertia correction you will find that there is a cutoff point in the frequency which of course in actuality doesn't exist. This is a spurious phenomenon that is caused by the introduction of an additional correction term which though in reality there, should be omitted. The reason for that is of course more complicated phenomena had been left out and there is some kind of counter balancing, so one has to be really careful in adding approximation terms to some of these analyses. Dr. Hoppmann's paper is open for discussion.

FROM THE FLOOR

Dr. Hoppmann, you made numerous references to the term "normal mode analysis" stating that it would work, but you seemed to put a question mark at the end of it all. One of the impulsive load problems we are facing is the effect of the high pressures and short duration loads and the fact that you do generate waves from extremes, such as spalling. Are these things sort of coupled in with the normal mode response or can we just ignore this and still go, on the assumptions about the structure, and not just about the materials, and get a good estimate of response by ignoring the wave propagation phenomena?

DR. HOPPMANN

No, that was one of the points I was trying to make, to suggest just that. I don't think it was accidental in the history of the subject. For example I think it was a problem many years ago. Notice the normal mode analysis of vibrating screens before Fourier developed the theory of functions. Now this isn't the nature of it. The thing that impresses me is that with the real structure (and I have vibrated many of them, all different kinds) now you actually find the modes. They are a reality, you see. We must admit, if I take a plate and I strike it a blow at a point, it must be that a wave of finite velocity emanates from that point. It would defy sanity to say otherwise. But somehow or another it all straightens itself out so that ultimately it partakes of the normal mode type of excitation. You can add up the effects of your normal mode and predict fairly well the total deformation of the structure. This is actually what happens. You see the two normal modes I added on the board were almost on top of each other. To my mind this is fantastic, to ask nature to do this. You calculate it this way and it comes out this way, which means it is not accidental. The normal mode is infinitely tied up with the total of our vibrations but I will admit that more has to be done. That is the implication that I tried to give; starting with the wave transmission theory to tie those two together better is somebody's problem in the future. But it seems to me it is an extremely complex thing.

DR. GOLDSMITH

Any other questions?

FROM THE FLOOR

I hate to ask a dirty question here, but what happens when the structure is no longer linear?

DR. HOPPMANN

That's not a dirty question; you are just tripped up in your psychology. All these problems are manifoldly complex. One should notice that the spectra of velocity that we have discussed here are enormous. You see if I am interested in a blue suit, don't tell me that you can buy a red one. We know there are red suits. All of this fits together, and we know for example in ships, with which we are all familiar, that if you get struck in a certain point, you evaporate some of the metal. Nobody talked about evaporation of metal in a molten state, but this happens. As you proceed away from it, you can see the varying degree of deformation, but if you happen to be standing at one of those points you will certainly be concerned about the nature of vibrations at that particular point. I don't know where the dirty got into it.

FROM THE FLOOR

Can you tell us about vibrations?

DR. HOPPMANN

I don't pretend to; I'll leave that to Dr. Dorn.

DR. GOLDSMITH

Well, I would like to again thank Dr. Hoppmann for a very interesting presentation; not only interesting but an illuminating one as far as concepts on what is dirty and what is clean.

Before closing this session, I would like to remind you that the evening session will start promptly at 7:00 o'clock. Before adjourning the meeting I will turn the microphone over to Colonel Standifer for further remarks.

COLONEL STANDIFER

I really appreciate Dr. Hoppmann's approach, and I want to encourage all of you in the next day, throughout the rest of the session, to think about what you've heard and try to give us challenging possibilities of real valid research. What are the things that we should do in the order of priority? What are the things we should put major emphasis on, and long

range emphasis on? These are the things that we are going to be talking about tomorrow afternoon. Again I particularly want to thank Dr. Hoppmann for delineating in the field, particularly in the field of structures where our problems lie. The Symposium is recessed until 7:00 o'clock tonight.

TECHNICAL SESSION III

HYPERVELOCITY IMPACT

Raymond Bisplinghoff, Ph.D.
Session Chairman

National Aeronautics and Space Agency

TECHNICAL SESSION III

INTRODUCTORY REMARKS

COLONEL L. R. STANDIFER

I think this next session is vitally important to all of us. It's something that is in the far unknown. In most cases we don't even know what is going to be shooting at us. I think that it is an area that shoul' be defined.

I take great pleasure in introducing the chairman of this session on Hypervelocity Impact, whether it is weapons effects or whether it is the meteoroid of less than several micron size. This is Dr. Raymond Bisplinghoff, Director of the Office of Advanced Research and Technology, National Aeronautics and Space Agency (NASA). He attended the University of Cincinnati right down the road here and has degrees in aeronautical engineering, an M.S. degree in physics, and a Science Doctor from the Swiss Federal Institute of Technology. He's a Fellow of the American Academy of Science, Royal Aeronautical Society, member of the Learned Societies of America and the world of Sigma Xi. He has some 40 papers in the general field of structural mechanics, aeroelasticity and aeronautics. He was formerly the head professor and the department head of the Department of Aeronautical and Astronautical Structures Research Laboratory at MIT, and as such I would suspect has very eminently been associated with Air Force programs and requirements, particularly for advanced education. At this time I take great pleasure in introducing Dr. Bisplinghoff.

DR. RAYMOND BISPLINGHOFF

Chairman, Session III

Thank you, Colonel Standifer. Lady and gentlemen. We have three after-dinner speakers tonight and their subject is hypervelocity impact. As long as we are faced with the prospect of having three after-dinner speakers on this subject, I don't think we could have picked three better qualified speakers.

To many of us who have been in the academic community this subject is one of considerable interest from a purely academic standpoint. It has had this appeal for many years and, of course, more recently, it has had an appeal from the practical side from the standpoint of ballistic missile defense and penetration by meteoroids. This latter problem, of course, has come into focus recently because of the space program. As you all know, the possibility of penetration by meteoroids represents one of the most serious problems confronting the space vehicle designer.

Tonight, we are going to treat the subject from three sides: the theoretical side first, then the experimental side, and finally, the side which represents the devices which project particles in the laboratory. The speaker who will present the theoretical side is Dr. R. L. Bjork of the RAND Corporation. Dr. Bjork has been a pioneer in this field and I think those of you who have followed it will recognize his name immediately. He was educated at the University of California, Berkeley, UCLA, and the University of Utah where he received his Bachelor's degree in 1948. He did three years graduate work on solid state physics at Cornell and then went to the RAND Corporation where he has been

for nine years. He has published a great many papers on hypervelocity impact and nuclear cratering. It gives me great pleasure to introduce Dr. Bjork.

DR. R. L. BJORK

Thank you very much for that nice introduction. I began working on the problem of hypervelocity impact shortly after the RAND symposium in 1955. I had a specific goal, namely, to calculate the effect of hypervelocity particles on re-entry vehicles and the possible effect of meteoroids on spacecraft. I immediately went to a particular formulation of the problem, namely, a hydrodynamic problem, and made a calculation of the phenomenon on that basis. I'll discuss the results as well as some extensions with you tonight, and make a comparison with theory.

STATUS OF THEORY

by

R. L. Bjork, Ph.D.

RAND Corporation

PAPER NOT AVAILABLE AT TIME OF PUBLICATION

DISCUSSION

DR. BISPLINGHOFF

Thank you very much, Dr. Bjork. Are there any questions anybody would like to ask?

DR. GOLDSMITH

I wish to address some questions to Dr. Bjork and the first of these questions concerns some papers published about five years ago in which it was shown that the depth of the penetration resulting from high velocity impacts of spheres on the wax targets actually decreased with higher velocities because the projectile broke up and eventually, I presume, vaporized. I am speaking of the depth of the penetration, not necessarily the crater. It would seem to me that a correction of this kind would have to be made in the impact processes described here. And the second comment or question I would like to address to Dr. Bjork is whether or not he has compared the results of this work to the theory proposed by Mel Cook in a paper I believe that was published in the Journal of Applied Physics in 1959, in which he considered energy of vaporization, the energy of heating, the energy of fragmentation, and so on, with the crater volume and crater depth.

DR. BJORK

Well, I'll answer your first question first. Now, the results you ascribed to Hughs and . . . (inaudible) . . . were not restricted merely to wax. This is a general feature of many target materials. I should probably draw a diagram. (Drawing on board.) A lot of materials show this particular feature, that if you plot penetration versus impact velocity (I'll plot the logarithm here), as you go to very low velocity in the ballistic range, the penetration increases with about the four thirds power of velocity. Then, in this region, the projectile actually remains intact, sort of flying through the medium. Then, as you reach a certain point where the dynamic pressure induced on the impact is greater than the strength, the way a lot of people describe it, the projectile fragments, and the result actually becomes erratic here. In some cases, the pressure goes down; in some it remains about the same but the volume is increasing, but then it becomes regular again and begins to go up. Now, this hydrodynamic model, of course, is only applicable to this region above the velocity where the projectile is fragmented. I have the projectile just flowing, like a fluid. This is quite an interesting regime. As a matter of fact, at the first RAND symposium, I think someone was mentioning the fact that using the tungsten carbide you get about 14 calipers of penetration at a certain velocity, but when we increased velocity with the same core, it would then fragment on the surface. This is a very dramatic transition when the projectile begins to fragment.

The answer to your second question is "no," I haven't compared the results. These results of mine were published in 1958 and Cook's came out later, in 1959, you say, and I haven't made a comparison.

ABRAMSON, SOUTHWEST RESEARCH INSTITUTE

This afternoon, Dr. Dorn told us about how the very small, but finite times required for dislocations to begin moving could be important in other problems. I wonder here if there

is any influence of the small but finite time required for a phase change to actually occur. Since your theory depends somewhat heavily on the process of melting, I wonder if finite times are important.

DR. BJORK

Well, the theory is based on the hydrodynamic model, of course, and as the input to this, I used the equations of state generated by Los Alamos. I'd like to point out the fact that I have no adjustable constants in here. I merely put in the equation of state as I find it. I put in the initial conditions and then I solve the problem. The answer about whether the dislocations have the effect--I would suspect strongly it is "no," and the reason for that is this: Los Alamos workers and others measured the rise times of these shots and they find these to be on the order of ten to the minus seven seconds almost uniformly, and it turns out that the details in this rise time are not very important; that is, you can put in a viscosity that will smear out the rise times a little bit, but the material will always come to some point on the Hugoniot, independent of the shot structure--the track it actually traces in getting up to this final stage--so that should not influence the process.

DR. BRODE, RAND

If I could hazard an amplification of that response. In the thermodynamic sense, what you treated is under the presumption of equilibrium, that any rate effects would then be in violation of this equilibrium. Is this presumption of thermodynamic equilibrium correct in this case? I think that at these kinds of temperatures there are no known reaction rates which would leave you out, seriously out of the thermodynamic equilibrium with any significant times for propagation of shots. At lower temperatures, where you have these dislocations to be concerned about it really doesn't matter.

DAVE LOWE, GEOPHYSICS CORPORATION

I'm wondering whether in sheer flow, such as the material riding up the side of the crater, if you get additional heating of the material which might cause it to liquefy even if it were initially below the state in which it would provide you direct passage of normal shot.

DR. BJORK

Yes, I'm sure you would. That is, the material is conditioned and then a flow occurs in which there is a great deal of sheering. Certainly, things like friction will heat the material some more. Now, I'd like to point out that this process is not understood in detail. My predictions, if you recall, gave an error of plus or minus 10 percent. The details of these last stages of crater formation have not been treated satisfactorily by any theory that I know of. You would expect more heating, and exactly what goes on in that final stage is as yet unknown. I think that to understand them one should really note that the hydrodynamic model goes to pot, that is, the assumption becomes bad in these later stages and the correct way to treat it would be to do an elastic and plastic calculation. This seems to be beyond anyone's capabilities at the present time.

BERRY, GENERAL ELECTRIC

Has anything been done on the morphology or the topography of these craters so that experimentally it could be turned into a verification of the theory?

DR. BJORK

When I made the calculations, I noted that the geometry of the crater was essentially hemispherical. Remember, I was calculating at five and a half kilometers a second at the time when people were only getting three and four, and the experimental craters do remain hemispherical, roughly.

BERRY

Specifically, what I was referring to was more on a microscopic sense in looking at the crystalline configurations, and any types of flow markings, etc., in the craters, other than just the macroscopic spherical configurations that you might expect.

DR. BJORK

Oh, yes, well, there are many things. If you section one of these target materials, that is, you have a cross section of the crater that looks something like this (draws on board), there's a lip formed here. You will see lines that come up like this. They are distorted, fan out something like this. Now, this gives you some indications of the displacement that occurred outside the crater. The morphology of these lines can be explained by the displacement produced by the shock wave that I pointed out. Now, other studies have been done in which they etch the surface of these materials and study grain sizes to try to reduce the temperatures, etc. I believe that in things like lead, they find the grains are considerably enlarged, but I don't know how many other materials have been treated or how adequately.

DR. BISPLINGHOFF

Gentlemen, we'll have to break off discussion here and get on with our next paper. May we again thank you, Dr. Bjork.

We turn our attention next to a paper titled "Status of Experiments" where we will see a further comparison between theory and experiment. The authors are Walter Herrmann and Arfon Jones of MIT and the paper will be given by Dr. Herrmann. Dr. Herrmann came originally from South Africa where he did all of his college work, receiving his Ph.D. there in 1955. I was a colleague of his for a good many years and he came into this work by way of an interest in shock tubes and did a great deal of work on unsteady aerodynamics before he became interested in wave propagation in solids and hypervelocity impact. May I present Dr. Walter Herrmann.

DR. WALTER HERRMANN

Thank you very much. There has been a great deal of experimental information generated since the first RAND symposium on hypervelocity impact where attention was focused on this program, and there is a great deal of rather miscellaneous information which is as yet unrelated to theoretical treatments, or for that matter, to other pieces of experimental information. It is not the intention here to summarize all of this information but rather to show the development of experimental work, the general trends, and where they are leading us. Now, Dr. Bjork has approached the problem from a theoretical point of view, and looking simply from an experimental point of view, we will arrive at some of the same conclusions. Of course, ballistic penetration has been of interest to the military for a very long time.

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HYPERVELOCITY IMPACT STATUS OF EXPERIMENTS

by

Walter Herrmann, Ph.D. and Arfon H. Jones, Ph.D.

Massachusetts Institute of Technology

HYPERVELOCITY IMPACT

STATUS OF EXPERIMENTS

Walter Herrmann
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ABSTRACT

A brief account of the development of hypervelocity impact experimentation is given, and the applicability of the presently available information to target design is discussed. Most of the available information refers to cratering in effectively semi-infinite targets, or penetration of a single thin target, at velocities below 10 km/sec. This information is not directly applicable to hyperballistic penetration of complex built up targets, nor to penetration at meteoroid velocities. Dynamic measurements of quantities during crater formation, however, have led to a detailed qualitative understanding of cratering physics, which should ultimately lead to an adequate theory which will be useful as a design tool, and allow rational extrapolation to higher velocities.

HYPERVELOCITY IMPACT
STATUS OF EXPERIMENTS

INTRODUCTION

Since the First Hypervelocity Impact Symposium held at the Rand Corporation in 1955,[1] a great deal of attention has been devoted to experiments in hypervelocity impact. The theoretical understanding of the phenomenon is still far from complete, and much of this experimental information is still unrelated. It is not the intention of this paper to attempt to summarize the available information. This has been done elsewhere.[6] Rather it is intended to trace the development of experimental work, and to define the current status.

SECTION I: HISTORICAL NOTE

Ballistic penetration has been of vital concern for a very long time. Conventional guns are limited to muzzle velocities below about 2 km/sec. In this range, penetration of targets is accomplished by the familiar modes of plugging or petalling, brittle penetration and spallation, depending on the velocity and the condition of the target. It was recognized that target penetration was less effective when the projectile broke up, and ballistic designers strove to maintain projectile integrity during penetration.

During the Second World War, two new developments occurred which allowed attainment of much higher projectile velocities. Discovery of the Munroe effect led to the development of shaped charges in which converging detonation waves in H.E. are used to collapse a conical metal liner. This results in the ejection of a thin jet of metal along the axis of the liner at velocities up to about 10 km/sec. Subsequent development led to modifications in which integral pellets could be projected at velocities up to about 6 km/sec.

The other development, originating at the New Mexico School of Mines, was that of the light gas gun. By employing a heated compressed light gas instead of heavy powder gases to drive the projectile, base pressure on the projectile could be maintained much longer, and much higher muzzle velocities could be attained. The light gas was compressed and adiabatically heated by an explosively driven piston in a pump tube. Initial firings yielded muzzle velocities in the neighborhood of 4 km/sec. Recent refinements of light gas gun design have made it possible to exceed a muzzle velocity of 10 km/sec.

Firings at high velocities revealed rather startling behavior. Projectiles were severely deformed or, if brittle, were shattered. Penetration increased with velocity much more slowly than expected from the low velocity work, or if the projectile shattered, penetration actually decreased sharply. At high impact velocities, the projectile and target appeared to flow like a liquid, and wide hemispherical craters were formed, which were quite unlike the narrow holes produced at lower velocities. The name hypervelocity impact was given to this phenomenon. The suggestion of fluid flow prompted the name hydrodynamic cratering. No rigorous definition of these terms has ever been widely adopted, and their use often leads to some confusion.

In the early stages, there was little interest in hypervelocity impact from a military standpoint. A number of firings were carried out against very thick targets, and peculiarities of the craters were noted. The real impetus came when it was realized that hypervelocity impact was of crucial importance in ballistic missile defense, and the protection of space vehicles against meteoroids. An ever increasing number of laboratories began developing hypervelocity projectors and gathering cratering and penetration data.

Nearly all of the early work involved firings into thick, effectively semi-infinite targets, and in many cases the data gathering was incidental to gun development work. Each laboratory attempted to correlate its own data with a suitable correlation expression, usually based on a simple power law. In many instances these expressions appeared contradictory. Designers used these expressions and produced a variety of missile vulnerability estimates and estimates of the probability of meteoroid penetration which varied by several orders of magnitude, depending on the correlation expression which they chose to extrapolate, and the assumptions they made in converting the semi-infinite cratering data to apply to thin targets.

In the last two years more emphasis has been placed on penetration of thin or built up targets of more practical interest. More emphasis is also being placed on basic measurements of quantities such as the stress and deformation fields surrounding the crater during formation rather than a simple post mortem observation of crater dimensions. This latter approach should ultimately lead to a deeper understanding of the cratering process, and when combined with theoretical development, should allow a more rational approach to the protective design of structures, or optimal design of projectiles.

SECTION II: SEMI-INFINITE TARGETS

The available mass of information on hypervelocity impact was collected and studied in detail in 1961 by the present authors.[6] Most of the information then available referred to cratering in effectively infinitely thick targets.

Most of the correlation laws devised by the various laboratories were based on velocity scaling of the type

$$\frac{p}{d} = kV^n \tag{1}$$

where p is the depth of crater, d the projectile size, V the projectile velocity and k a dimensional proportionality factor. Irwin[1] had long ago noted a historical trend for the exponent n to decrease as higher velocities were obtained, from a value of 2 given by Robins and Euler in the early eighteenth century to a value of 4/3 given by De Marre, and in wide use in conventional ballistics two centuries later. This trend was observed to continue into the hypervelocity regime. For ductile projectiles, as the velocity increased, more and more severe deformation of the projectile occurred until the projectile appeared to be plated over the surface of an almost hemispherical crater, or was ejected from the crater altogether. Workers at N.A.S.A. Langley and elsewhere fitted their data in the range 2-4 km/sec. with an exponent $n = 1$. Elsewhere data in the range 3-7 km/sec. was fitted with an exponent $n = 2/3$.

The latter value, i.e. $n = 2/3$, became one definition of the hypervelocity regime. If craters are assumed to be truly hemispherical in this range, which in fact is not always true, then it is easy to see that $n = 2/3$ implies that the crater volume is proportional to the kinetic energy of the projectile. The simplicity of this concept led to the assumption that this relationship would be true also at much higher velocities, and many extrapolations were based on this idea.

The trend for n to decrease with increasing velocity suggested that a power law in fact gave a very poor representation of the data, and the present writers found that a two parameter logarithmic dependence of penetration on velocity could represent the data for ductile projectiles very well over the entire velocity range from 1.5 km/sec. to 7 km/sec., i.e.

$$\frac{p}{d} = k_1 \log_e (1 + k_2 V^2) \qquad (2)$$

where k_1 and k_2 are constants depending on the projectile and target material. An example plot is given in Figure 3. Such a logarithmic law is actually equivalent to that postulated by Poncelet in 1829 on the basis that the resistance to penetration is proportional to the square of the instantaneous projectile velocity, i.e. was due to inertial resistance of the target.

For brittle projectiles, a departure from the smooth monotonic increase of penetration with velocity was observed (Figure 2). The region of dependence of penetration on the four thirds power of velocity was extended up to a velocity where the projectile was observed to shatter. The sudden increase in presented area of the projectile then led to a sudden decrease in penetration at higher velocities. Nevertheless, the lower and higher velocity data lay on the appropriate logarithmic curve, with an intermediate hump due to the projectile strength and shattering effect.

An extrapolation of the logarithmic expression indicates that at still higher velocities the penetration could be approximately represented by a power law of the form of Eq. 1 with $n = 1/3$. This is in substantial agreement with the theoretical calculations of Bjork.[3] It can be seen that $n = 1/3$ is equivalent to a proportionality between crater volume and the momentum of the projectile, and this idea has also found some proponents. It is, however, unlikely

that any significance can be attached to a dependence of crater volume on either projectile kinetic energy or momentum in certain velocity ranges.

Clearly, extrapolations of Eq. 1 and Eq. 2 to much higher velocities will not be in agreement. This is illustrated in Figure 3, which shows a log-log plot of penetration for aluminum projectiles and targets in which straight lines represent a power law of the form of Eq. 1. Extrapolation to higher velocities clearly leads to very large disagreement. So far, no definitive experiments have been forthcoming to indicate which extrapolation is more realistic, and there is at present no rational basis for extrapolation of penetration data above 10 km/sec. Expressions such as Eq. 1 and 2 are thus primarily useful as interpolation formulae within severely restricted velocity ranges.

The uncertainty can be resolved only by experimental firings at much higher velocities. Light gas guns appear to have been developed to a point near their limit of performance. Velocities near 20 km/sec. have been achieved by high explosive means, but the projectile mass, shape and condition are difficult to define. It was early anticipated that small particles might be accelerated to very high velocities by electrical means, i.e. either by electromagnetic or by electrostatic means, but results so far have not been very encouraging. The most promising means for accelerating very small projectiles seems to be to use an exploding wire to produce a plasma which is used as the propelling fluid, and some very high projectile velocities have recently been claimed for such a device. At the time of writing, however, no definitive experimental results are available to resolve the uncertainty in extrapolation, and there are no reliable means of estimating penetration above about 10 km/sec.

Attempts had also been made to scale the proportionality constant in Eq. 1 with projectile and target material properties, and a conflicting variety of expressions were constructed at various laboratories. One of the earliest ideas first proposed by workers at Utah University was that the elastic sound wave velocity in the target played an important role, and that an abrupt change in cratering mechanism might be observed as the impact velocity went through the sonic range. Such a phenomenon was never observed. Many correlation expressions based on the target sonic velocity had been proposed, but on careful re-examination it was found that data covering only a few special materials could be fitted by these expressions.[6]

It had also been surmised that the target strength would play an insignificant role in the hypervelocity regime. However, Pugh and Eichelberger[1] had noted a dependence of the crater size on target hardness as early as 1955 in experiments with shaped charges. Careful examination of the collected data revealed a dependence of crater size on target hardness up to the highest velocities achieved experimentally, i.e. 7 km/sec. Sufficient data existed so that the effect of projectile and target density and target strength could be determined separately. A rough correlation of the constants k_1 and k_2 with projectile density and target density and strength was found, so that the data could very approximately be represented by the expression

$$\frac{p}{d} = 0.6\ \kappa^{1/2}\ \log_e\left(1 + 1/4\ \kappa^{2/3}\ B\right) \qquad (3)$$

where κ is the ratio of projectile to target density and B is the Best Number, defined as

$$B = \frac{\rho_t\ V^2}{H_t}$$

where ρ_t is the target density and H_t is the projectile Brinell Hardness (in appropriate units). The Best Number therefore roughly represents the ratio of inertia forces to target strength.

Over a much more restricted velocity range* the data could also be represented approximately by an equivalent power law as follows:

$$\frac{p}{d} = 0.36\ \kappa^{2/3}\ \ B^{1/3} \qquad (4)$$

The fact that Eq. 3 and 4 are very approximate is hardly surprising. We have attempted to represent the complex dynamic strength and compressibility properties of the materials by simple static properties, and have furthermore placed a severe restriction on the functional form of the expression. The many complex phenomena occurring during cratering, which have been described in detail elsewhere[6,3,4] suggest that a much more complex functional relationship will be required for an adequate representation. That some success is achieved by a simplified expression of the type of Eq. 3 or 4 indicates that there is some correlation between static and dynamic properties of the materials.

* The ranges of validity of Eq. 3 and 4 are strictly limited to the velocity ranges covered experimentally, and these differ for various material combinations.[6]

It appears that the data is insufficient to allow construction of a more complex empirical expression, even given that suitable dynamic properties of the materials could be defined and evaluated, and further development must await further theoretical advances.

In the last two years several excellent studies have been reported in which high speed framing camera pictures and X-ray shadowgraphs were taken of craters in the process of development.[4,5] These show details of the cavity growth, shock wave propagation and spray issuing from the crater, which confirm qualitatively the pressure contours and velocity vectors computed theoretically by Bjork.[3] Recently, several studies have been initiated in which attempts are made to measure instantaneous displacement and stress fields around the growing crater. For example, Frasier[5] has imbedded wires in a wax target. Motion of the wires during cratering in a magnetic field provided by external field coils then provides an electrical signal which allows measurement of shock and material particle velocities. These in turn may be converted to pressure and density information through the use of the Rankine Hugoniot equations. Piezoelectric and capacitance pressure gauges are also being investigated for use on targets. While such measurements are very difficult, the resultant information should eventually allow the construction of a rational theoretical model, and are thus of vital importance.

SECTION III: THIN TARGETS

While cratering in effectively semi-infinite targets is of great interest, some of the most important questions concern the ballistic limit of thin targets, i.e, conditions under which the target is just penetrated; and in the case of penetration, the mass, velocity, distribution and condition of the material proceeding through the target, and its capability of inflicting further damage on interior structure. This information cannot easily be inferred from data on crater sizes in semi-infinite targets.

Among the very few early systematic studies of penetration of thin targets was one carried out at the N.A.S.A. Langley,[7] since declassified, in which the ballistic limit of

aluminum plates was observed, and in which steel and aluminum projectiles were used at velocities up to 4 km/sec. For the particular conditions of these tests, it was observed that targets were just penetrated that were 1 1/2 times as thick as the crater depth produced by a similar projectile in a semi-infinite target. Since there was very little other information available at that time, this observation was quickly generalized and used by designers to convert semi-infinite cratering data to ballistic limits at any velocity.

At conventional ballistic velocities, the projectile usually remains substantially intact during penetration. By contrast, it was found that at hypervelocities the projectile shatters, and projectile and target material are emitted from the back of the target plate in a more or less finely divided spray, depending on the velocity (see Figure 4). The critical velocity at which the projectile shatters is a function of target thickness and material. The condition of this spray, i.e., velocity, particle size, and spatial distribution, is much more difficult to measure than the final crater size in an infinitely thick target, and most of the early experiments in thin target penetration were more or less qualitative. Most of this work consisted of firing at a series of thin plates spaced some distance apart, and observing the number of plates perforated, or observing the qualitative damage to a thick witness plate spaced some distance behind the thin target plate. While such experiments have rather limited value, they have nevertheless led to a qualitative understanding of the physical phenomena occurring during penetration.

During the past year, several laboratories have switched attention from semi-infinite targets to thin targets. Several excellent studies are under way in which high speed photography, X-radiography and other techniques are being used to determine the characteristics of the spray, and its potential to inflict further damage on interior structure. Results of most of these studies have not yet been published. However, it is to be expected that when results of these studies are correlated and compared, a reasonably detailed quantitative understanding of thin target penetration below 10 km/sec. will emerge. As in the case of semi-infinite targets, extrapolation of these test results to meteoroid velocities lacks experimental or theoretical justification, unless experiments at much higher velocities can be carried out.

Many orbital or re-entry vehicles are composed of complex multilayer built up structures. Penetration theory is currently inadequate to deal with such structures, except to provide qualitative guidance to target design. It is clearly not possible to generate empirical data covering all possible structural permutations. Target design and vulnerability analyses of such structures must therefore, as in the past, rely heavily on testing of geometrical scale models, using the correct anticipated projectile velocity.

SECTION IV: SUMMARY

The capability now exists to carry out carefully controlled impact experiments at velocities up to 10 km/sec. Several techniques show some promise of providing projectile velocities twice as high, but require some further development before projectiles with defined and controllable shapes and masses can be produced.

The greatest bulk of existing experimental data refers to crater sizes in effectively infinitely thick targets. Several empirical <u>interpolation</u> formulae have been produced, but when these are extrapolated to high velocities they lead to large disagreement in the predicted penetration. There is currently no rational theoretical or experimental evidence to indicate which of these expressions will be more realistic. There is therefore currently no information available on cratering at meteoroid velocities.

Only extremely rough scaling laws from one material to another have been found using static material properties. On the other hand, high speed photography and other techniques have provided an excellent qualitative understanding of the physical phenomena occurring during penetration. This should provide a firm basis for a rational theoretical treatment of cratering.

There is relatively much less information available on impact at oblique angles, the effect of projectile shape on cratering, and penetration of thin targets. While again an excellent qualitative understanding of the penetration

mechanisms has been gained for these cases, there are important gaps in the quantitative information available. Some of these are currently being closed.

The available information is not directly applicable to penetration in complex multilayer built up targets, and such target design must, as in the past, rely heavily on testing at full scale velocity.

Nor is the available information applicable to meteoroid velocities. Extrapolation to higher velocities is very uncertain, and current estimates of meteoroid damage must be considered speculative.

However, it is expected that the insight into cratering mechanisms gained in carefully instrumented and controlled laboratory experiments on the simpler target configurations will advance theoretical development to the point where it will become a useful tool in complex target design, and lead to a rational basis for extrapolating current data to meteoroid velocities.

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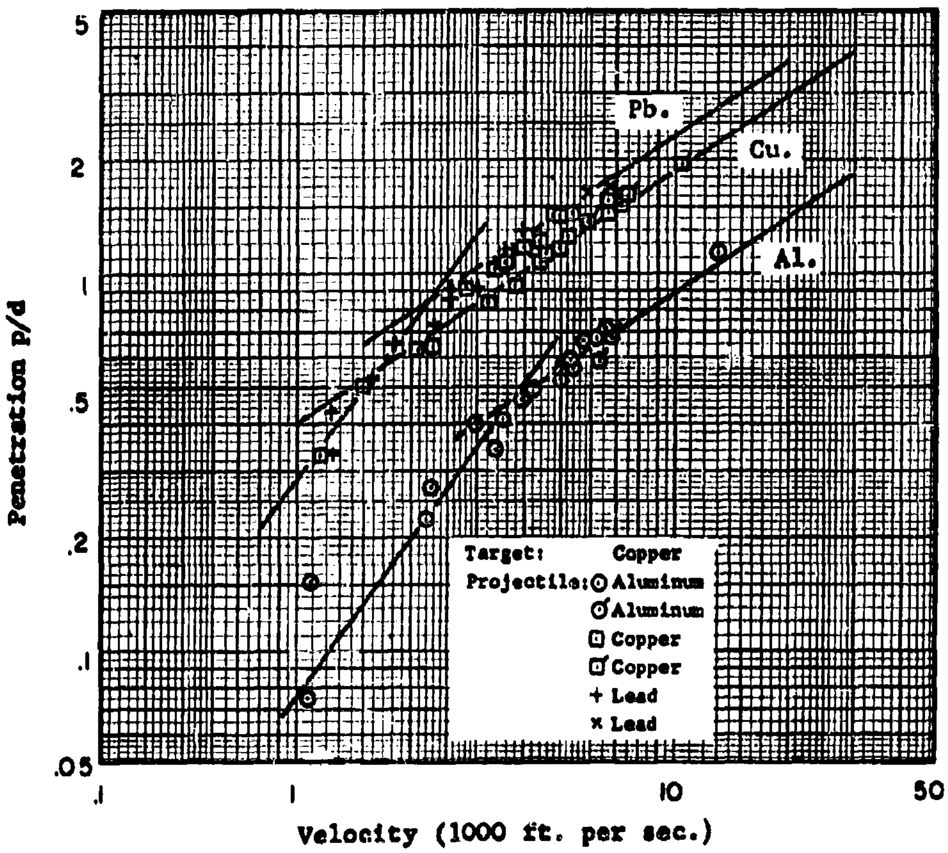

Fig. 1 Penetration in Copper Targets

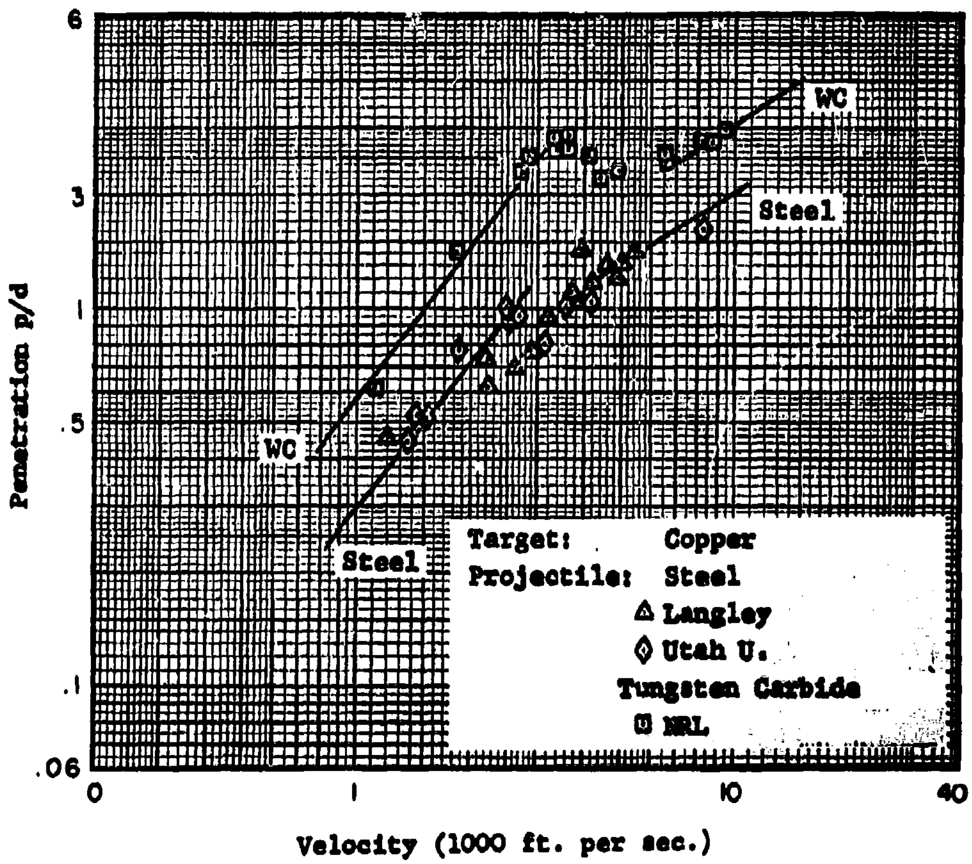

Fig. 2 Penetration in Copper Targets

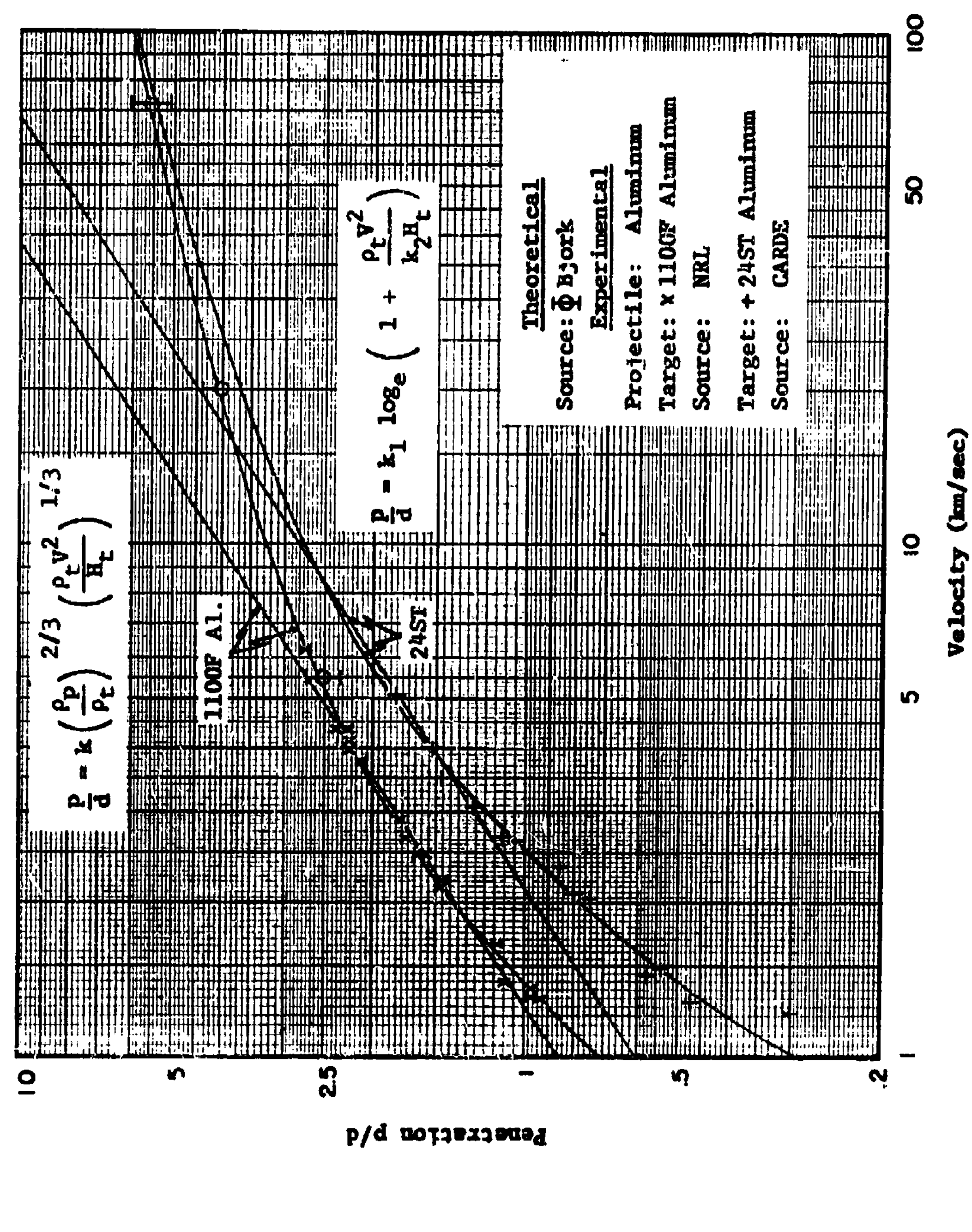

Fig.3 Extrapolation of Empirical Penetration Laws for Aluminum Projectiles and Targets

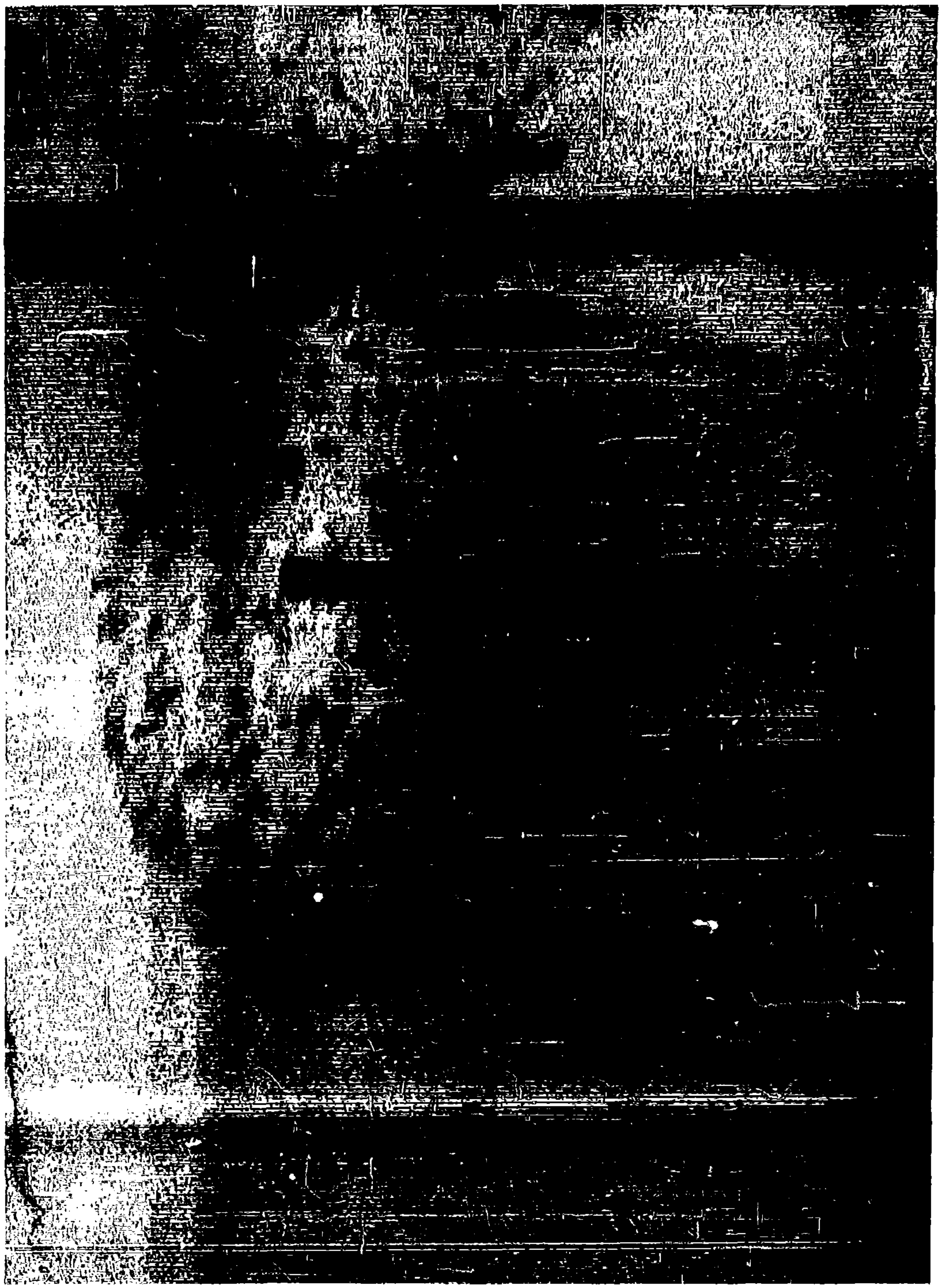

Fig. 4 - Framing camera photograph of the spray caused by a projectile on perforating a thin plate target (0.22 in aluminum sphere impacting a 1/8 inch aluminum target. Impact velocity 13,400 ft/sec.)

DISCUSSION

DR. BISPLINGHOFF

Thank you very much Dr. Herrmann. Are there any questions or comments?

MORTENSEN

I don't have a question; I just wanted to make a small correction to your statement as to shaped charges today. While it's perfectly true that end charges project single particles at six kilometers per second, there are regularly fired special shaped charges at ten to twelve kilometers per second.

DAVIS, HUYCK CORPORATION

In listening to your presentation and the preceding one, I get the feeling that we are missing the key parameter. We talked in terms of velocities; we talked in terms of Brinnell hardness; we talked in terms of density ratios. Are we not skirting around the basic issue of time? In other words, we have a certain time of loading; we have a certain time of reaction to the loading. When one crowds the other, you get a change. As you start moving up the velocity spectrum, really what you are changing is the time parameters of the reaction. I wonder if it wouldn't be possible to formulate this same general composite of empirical approaches based on time rather than all these other things. Has anybody given any thought to this?

DR. HERRMANN

No, I think the prime parameter which is changing is the pressure. At the higher velocities you are introducing very much higher pressures and therefore you are entering into a different regime from that point of view. Now, the reaction of the material is different in the different ranges. We are not talking here of elastic-plastic ranges, but at the very much higher velocities; we are actually in a range where the fluid is at extremely high energies, at least initially. Now the afterflow is affected by strength and here, perhaps, rate effects are important. Now, this refers to the experimental range of velocities up to 10 kilometers and perhaps a little higher where there is an observed difference in crater size with strength. I would agree with you that here time is important through the rate effect which doesn't allow us to define the strength parameters of the target correctly. We are trying to define the strength parameters through static strength and this is undoubtedly incorrect.

DR. DRUCKER

About ten years ago, I suggested that perhaps metal cutting data would be more suitable to use than any static data such as Brinnell hardness. Has anyone ever tried this to see if it works any better? . . . (inaudible) . . . sheer stress on the sheer plane in metal cutting turns out essentially, even at very low rates of cut, to be quite independent of the (speed?), and yet at very much higher volume (value?) than the static volume (value?) . . . (inaudible) . . . sheer stresses. Does this do any good? Anybody ever tried it?

DR. HERRMANN

Not to my knowledge. It may well be an answer. I think perhaps the heavier is very, very complicated, as Professor Dorn has indicated where the strain rate effect is not constant for different materials, and in different velocity ranges. This question of the dynamic strength might indeed be quite complex.

DR. PLASS

Are there any experiments on projectiles striking piezoelectric materials? If so, what is the effect?

DR. HERRMANN

I don't know of anyone that has done cratering experiments and observed the resulting piezo effect. It might be quite interesting to do this, but also it might be rather difficult to interpret.

LOWE, GEOPHYSICS CORPORATION

I notice on the correlations you presented on the board, there were only one or two points above ten thousand feet per second, and yet many people are shooting at above 20 and even 30,000 feet per second. How does this data fit? It seems to me it would be very interesting to see it on that plot.

DR. HERRMANN

These fits were obtained last year. Some preliminary data was available to us at that time between 20 and 30,000 feet per second and particularly in aluminum. We were not able to publish that data at that time because it hadn't been released by the labs, but this data was taken into account. What data we had seen, and there isn't very much in that velocity range, falls pretty much on our curves. I might state that the empirical fits are obtained to something like 1400 data points, collected over . . . (inaudible) . . . ranging from projectile-target material combinations which I think was something like 53, not all of which had a wide velocity range; most of this data was in the low velocity range. However, high velocity data that we have seen falls right on the curves.

DR. BISPLINGHOFF

Any other questions or comments? If not, may we thank you again, Dr. Herrmann, for a very interesting paper.

The final paper is on "Projection Techniques." Because of the desire to reach meteoroid velocities a great deal of effort has been expended on raising the velocities of projection, and Mr. John Curtis of General Motors Corporation is going to tell us about this tonight. His paper is a joint paper with Mr. J. W. Gehring, also of General Motors. Mr. Curtis attended the University of Idaho where he received his Bachelor's degree in mechanical

engineering in 1943. His experience includes four years at the Ames Research Center of NASA, and one year at the U. S. Naval Ordnance Testing Station at China Lake. His duties have been very closely involved with design and operation of light gas guns and gun ranges during this period of time. May I present Mr. John Curtis who will speak on Projection Techniques.

MR. JOHN CURTIS

Mr. Chairman, lady and gentlemen. It has been a long hard day. I'll try not to bore you unduly. Each of the various organizations involved in hypervelocity experimentation has developed or adopted their favorite projection techniques. Competition between the groups is rather spirited and each may feel that their own projection techniques are the best. In stepping into the midst of this competition and rivalry, I feel a little like a steer being led to slaughter. Since I have some rather firm convictions about some types of projection techniques, I'll take the big step. I hope I can show you some of the reasons for my convictions.

As experimenters in high impulse loading, I am going to assume that you are more interested in the projectile than the projector, and also that you are interested in hypervelocity projectiles. I am happy to be able to report that it is now possible to accelerate projectiles to velocities in excess of 60,000 feet per second, if you let me specify the projectile. However, having worked with impact experimenters for some time, I find that oftentimes they like to specify the projectile and this complicates matters, because there is no universally satisfactory projector for all projectiles at all velocities up to 60,000 feet per second.

PROJECTION TECHNIQUES

by

J. S. Curtis and J. W. Gehring

Defense Research Laboratories
General Motors Corporation

PROJECTION TECHNIQUES

J. S. Curtis
and
J. W. Gehring

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General Motors Corporation

ABSTRACT

This paper reviews the techniques used to accelerate projectiles to high velocity for impact and similar studies. Each type of projector is described briefly.

Current performance capabilities are discussed, and it is found that light-gas guns are capable of launching projectiles of known and controllable shape, mass, material, and orientation at velocities near 30,000 ft/sec, that explosive techniques can accelerate certain projectiles of less well defined shape and mass to a velocity of near 70,000 ft/sec, but that electric guns have not yet launched sizeable projectiles at even the relatively low velocity of 10,000 ft/sec.

The future potential of high velocity projectors is discussed. It is concluded that light-gas guns and explosive devices are the most promising types for sizeable projectiles and the electrostatic accelerators and explosive devices are best for accelerating microparticles. Performance estimates indicate that light-gas guns may be capable of firing at velocities of 40,000 ft/sec and explosive devices at velocities of 80,000 ft/sec and possibly as high as 100,000 ft/sec.

PROJECTION TECHNIQUES

INTRODUCTION

Experiments which involve a study of hypervelocity projectiles, either in flight or at impact, require a knowledge of the characteristics of the projectile. The characteristics of general importance are the geometry of the projectile, its composition, and its velocity. The composition of the projectile is determined before acceleration and the velocity must be measured after acceleration. The geometry of the projectile can change during acceleration so two alternatives are presented. Either the geometry must be determined by measurements during flight or the acceleration must be controlled so that the geometry does not change during acceleration. If precise control of the geometry is necessary, the latter alternative is the only practical one.

A large variety of accelerators are in current use or are being studied for use as hypervelocity projectors. The projectile accelerations produced by these accelerators range from 100,000 gravities to 100,000,000 gravities or more. A maximum value of acceleration exists for each projectile above which the projectile geometry will change during acceleration. Thus for many experiments, the acceleration produced by an accelerator is important as well as the velocity which it is capable of producing.

In the discussion to follow, projectors will be classified into three groups according to the source of the accelerating force: electric guns, gas guns, and explosive devices. Since the magnitude of the peak acceleration produced by various projectors is one of their most important characteristics, each of the projectors to be discussed will also be placed into one of three categories with respect to projectile acceleration. These categories are defined as follows:

Low G------- up to 500,000 gravities
Medium G---- 500,000 to 5,000,000 gravities
High G------- above 5,000,000 gravities

This division is made because in that low G range, very sophisticated projectiles, including composite, winged, finned projectiles and even active telemetering projectiles can be fired. In the medium G range, size, shape and composition of the projectiles are quite restricted and only passive telemetering is successful. In the high G range, only the most rugged projectiles can be fired and composition may be severely restricted.

SECTION I: GENERAL DISCUSSION

Hypervelocity projectors use many different methods of producing acceleration. The limited variety of projectiles which each can fire makes it very difficult to evaluate the projectors and compare their performance. A valuable comparison of projector performance can be obtained, however, by taking advantage of one characteristic which all projectors have in common,which is the velocity of the projectile emerging from any of them is precisely

$$V = \int_{o}^{t} a \ dt \qquad (1)$$

When the projectile characteristics are used to evaluate the terms in this equation, a valuable insight can be obtained concerning the type and size of projector necessary.

The size, shape, composition, and strength of a projectile can be used to determine the maximum allowable acceleration which the projectile can tolerate without deformation. The required velocity of the projectile is determined by the experiment being conducted. Thus, the time necessary for acceleration can be specified approximately. Time and distance are related so that, knowing the time of acceleration, the distance over which acceleration takes place can be determined. This distance then defines one major dimension of the accelerating device. Conversely, if an accelerating device already exists, the maximum performance of this device with a particular projectile can be determined based on the projectile characteristics.

The magnitude of the quantities involved is illustrated in Figure 1 which shows the time and distance required to reach several velocities of interest at various values of constant acceleration. It will be noted that to achieve a velocity in the range from 30,000 to 50,000 ft/sec in a distance less than one hundred feet will require constant acceleration of the order of one million gravities. The plot also points out the limitations of some accelerators. For example: If a gun with a barrel ten feet long is used to fire a projectile whose strength limits the allowable acceleration to one million gravities, the maximum attainable velocity with such a combination is about 25,000 ft/sec regardless of the capability of the gun when firing more substantial projectiles.

While the performance of some projectors may be acceleration limited, other factors are also important. For example, performance of electromagnetic accelerators can be limited by projectile or sabot melting, and explosive devices may be limited by the reaction rate (detonation velocity) of the explosive. However, given that advances in the chemistry of high explosives will achieve higher detonation velocities, the projectile velocity will then be limited by the capability of the metal projectile to undergo the rapid deformation and still retain its physical integrity.

In spite of the limitations mentioned, explosive devices do provide projectiles with a wide range of velocities and masses as shown in Table I (Ref. 1). The range of conditions that can be obtained by further development, using proven principles and techniques are shown, in addition to the ranges covered by past and current experiments. Projectile materials are limited for the most part to metals, and at the higher velocities the choice of metals is restricted. However, the mass and velocities available cover the range of primary concern in space travel. For many experiments, particularly those concerned with a study of material properties under intense shock loadings, techniques have also been developed to accelerate massive flat plates to a velocity as high as 8,000 ft/sec. (Ref 2).

SECTION II: ELECTRIC GUNS

Electric guns are considered here to be those which accelerate a projectile by electromagnetic or electrostatic forces. In electrical discharge or exploding wire guns, the electrical energy is used to heat a gas which in turn drives the projectile. They are thus properly classified as gas guns and will be considered under that heading.

Electromagnetic Accelerators

Electromagnetic accelerators which have been investigated fall into two categories: the D. C. rail gun (Ref. 3), and the A. C. solenoid gun (Ref. 4).

The A. C. solenoid gun fires a conducting projectile along the axis of a coil or series of coils. The projectile is accelerated by induction and repulsion when a pulse of current is sent through a coil which is properly oriented with respect to the projectile. To provide continuous acceleration over a considerable distance, a series of coils are pulsed sequentially. The timing of these pulses is critical because the projectile must be in the proper position when the coil is pulsed if it is to be continuously accelerated.

The D. C. rail gun fires a conducting projectile or sabot which forms part of a conducting loop (until the projectile leaves the gun). The projectile is placed in contact with two or more rails and is accelerated when a large current is passed through the circuit. Good electrical contact must be maintained between the projectile and the rails while the projectile is moving.

While electromagnetic accelerators show theoretical promise, many problems have plagued the experimenters. Contact resistance, pulse timing or frequency control, projectile or sabot melting, and power supply problems have contributed to the failure of these devices to produce high projectile velocities. The highest reported velocity attained by an electromagnetic accelerator is less than 4, 000 ft/sec so they cannot accurately be classified as hypervelocity projectors.

It is interesting to note that the Second Hypervelocity Impact Symposium devoted one entire session to papers on electromagnetic acceleration. The proceedings of the Third Hypervelocity Impact Symposium contain two papers on electromagnetic accelerators, one of which is a survey paper, and not a single paper on electromagnetic accelerators was presented at the Fourth Hypervelocity Impact Symposium.

Electrostatic Accelerators

Electrostatic accelerators have succeeded in launching projectiles at hypervelocities. The size and composition of the projectile is critical, however, to their successful operation. Projectiles are accelerated in an electrostatic accelerator by placing an electric charge on the projectile and allowing it to fall through an electric potential difference. The final velocity V of a particle when accelerated through a potential difference E is given by equation (2)

$$V = \sqrt{E\,q/m}\ \sqrt{2}\ \text{meters/second} \tag{2}$$

where a particle of mass m kg carries a charge of q coulombs. It can be shown that the ratio q/m increases as the size of the particle decreases. Thus, the electrostatic accelerator is more suitable for very small particles of low density and requires very high voltages. Friichtenicht (Ref. 5) has reported a velocity of 14 km/sec for a sub-micron sized particle accelerated through a two million volt potential difference.

Increasing the velocity produced by an electrostatic accelerator involves increasing either the quantity q/m or the voltage. The upper limit of the charge which can be placed on a particle is determined by the properties of the particle, and in particular the charge is limited by the field strength at which electron or ion evaporation will take place. Frichtenicht has operated within about a factor of three of this limit and points out that the absolute limit can be approached only with considerable added difficulty. The other approach of course, is to increase the operating voltage which is practical but quite expensive.

SECTION III: GAS GUNS

For several centuries, guns have been almost the only source of projectiles for high velocity experimentation. It is only natural that the scientist should look to guns when searching for an accelerator for hypervelocity projectiles. It has long been apparent, however, that conventional powder guns would not greatly exceed a muzzle velocity of 8, 000 ft/sec. Dr. W. D. Crozier provided the key which opened the door to higher gun velocities with his invention of the light-gas gun shortly after the end of World War II. That this invention was indeed the key to higher gun velocities is evident when the maximum gun velocities are plotted as a function of time as shown in Figure 2. From 1948 to the present time, the maximum gun velocity has doubled each seven years.

In all gas guns, the projectile is accelerated by the pressure of the driving gas acting on the base of the projectile. Since pressure is a manifestation of the particle velocity in the gas, it is evident that if a continuous pressure is to be applied to the base of the projectile, the particle velocity of the driving gas must be significantly higher than the projectile velocity. The particle velocity in a gas is proportional to the square root of the temperature, T, and inversely proportional to the square root of the molecular weight, M. Thus a high velocity gun must have a driving gas which has a large value of T/M.

Many methods have been used to provide a reservoir of gas with the necessary large value of T/M. Hydrogen or helium are almost universally used as the propellant gas to keep the value of M low. High temperatures are obtained b y numerous methods, and these methods provide a convenient method of classifying light-gas guns.

Classification of Light-Gas Guns

A. Adiabatic Isentropic Compression

1. Free Piston
2. Caught Piston
3. Accelerated Reservoir

B. Adiabatic Non-Isentropic Compression

1. Shock Compression - Two Stage
2. Shock Compression - Three Stage
3. Explosive Compression

C. Non-Adiabatic Compression

1. Arc Discharge
2. Exploding Wire
3. Steam Heated

D. Combination Guns

Adiabatic Isentropic Compression

The adiabatic isentropic compression light-gas gun (Figure 3) is a piston compression light-gas gun which employs a heavy piston traveling at a maximum velocity which is well below the sonic velocity in the gas being compressed. It is desired to compress the gas to a high pressure and temperature without generating any shock waves in the gas which can reach the projectile and destroy it with the large pressure pulse associated with the shock reflection from the base of the projectile. In the free piston gun (Ref. 6), the loading conditions are adjusted so that the piston is decelerated and stopped by the pressure of the gas in the pump tube. A certain amount of control over the variation of reservoir pressure with time, and hence base pressure, can be exercised in this type of gun but an excess of gas must always be used to provide a buffer to stop the piston. The presence of this buffer reduces the effective compression ratio of the gun so the final temperature of the gas is lower than if the buffer were not present. Free piston guns are capable of velocities up to 25,000 ft/sec.

In the caught piston gun (Ref. 7), no buffer of gas is provided to decelerate the piston. Instead, a short tapered section is installed at the muzzle end of the pump tube and the piston is stopped by jamming into this taper. Reasonable control over the variation of reservoir pressure with time can be obtained, but the piston catcher is a semi-expendable item and must be replaced frequently. Velocities above 31,000 ft/sec have been obtained with a caught piston gun at the Naval Research Laboratory.

The accelerated-reservoir gun (Ref. 8 & 9) is similar to the caught piston gun in that the piston is stopped by a taper at the muzzle end of the pump tube. In the accelerated-reservoir gun (Figure 4), however, the

pump tube is connected to the launch tube by a long gently tapered section and the piston is constructed of a plastic material which is readily deformable but relatively incompressable. During the pumping process, the plastic portion of the piston is extruded through the taper and into the launch tube. Thus, a minimum amount of gas is necessary which allows a maximum compression ratio for a given gun geometry. In addition, the light-gas reservoir is given a significant velocity component which is unavailable in other type guns, and which increases performance. Accelerated reservoir guns are currently in use at Ames Research Center and at General Motors Corporation, Defense Research Laboratories, and both establishments have recorded velocities in excess of 32,000 ft/sec. Gun performance computations indicate that this velocity is not the maximum available with this type of gun, when using cold hydrogen, and that if the gas is pre-heated the velocity capability of the gun should reach 40,000 ft/sec.

Adiabatic Non-Isentropic Compression

The reservoir of high-temperature, high-pressure gas is produced in this type of gun by single or multiple reflections of a strong shock wave. A typical two stage shock-compression gun is illustrated in Figure 5A. A piston may or may not be used between the powder and the light gas. If a piston is not used, the reservoir is formed by the single reflection of the shock wave generated by the rapid burning of the powder. If a piston is used, multiple shock reflections between the front face of the piston and the end of the pump tube result in a higher pressure and temperature reservoir and yields correspondingly higher velocities.

If a third stage is inserted between the second stage and the launch tube as shown in Figure 5B significantly higher piston velocities can be realized. In effect, a light-gas gun is used to fire the piston in a light-gas gun. The higher piston velocity generates a stronger shock wave which, after multiple reflections, yields a higher pressure and temperature in the reservoir than is achieved in the two stage gun. Many guns of this type are being used (see Ref. 10, 11, & 12) and this gun is capable of velocities in excess of 25,000 ft/sec. The good velocity capability and ease of operation are features which make this type of gun popular. However, the strong shock waves which are present in the reservoir of the gun make it difficult to launch any but the most rugged projectiles.

Pre-heating the gas in a shock-compression gun will theoretically improve its performance. Anderson (Ref. 13) has reported that the performance of a multi-stage shock-compression gun was increased from 26,000 ft/sec to 30,000 ft/sec by pre-heating the hydrogen in the final stage to 700 degrees Kelvin.

The explosive compression gun (Ref. 14) is a type of shock-compression gun wherein the gas is compressed by a shaped charge of explosive. Several versions of this type of gun have been tested and have achieved modest success. Reservoir pressure is very high and control over the pressure variation is difficult. Only the most rugged projectiles can be fired successfully.

Non-Adiabatic Compression

Non-adiabatic compression guns rely, in general, upon the addition of heat to a constant volume reservoir to generate the high pressure and temperature necessary. The arc discharge gun (Ref. 15) and the exploding wire gun (Ref. 16) utilize electrical energy to heat and pressurize the gas. Very high reservoir pressures and temperatures can be obtained with electrical discharge guns and Scully (Ref. 17), using a small gun of this type, has reported a velocity of over 30,000 ft/sec with micron sized projectiles. The variation of reservoir pressure with time cannot be controlled easily and the electrical discharge guns must be classified as high G accelerators.

The steam heated gun is a special case of the non-adiabatic compression type of gun. Oxygen mixed with an excess of hydrogen is burned to provide a heated propellant gas in a single or multiple stage gun (Ref. 18). Guns of this type have operated successfully at velocities below 20,000 ft/sec. The increased molecular weight of the propellant gas due to the oxygen present limits the velocity attainable with this type of gun. In addition the control of detonation has been very difficult in these guns, particularly those of larger size.

Combination Guns

Various combinations of the types of guns already discussed can be used to augment the performance of the basic gun. Of particular interest are the electrical augmentation guns. In one type of electrically augmented gun (Ref. 19), one or a series of electrical discharges are timed to occur immediately behind the projectile in the launch tube of one of the basic gun types. This electrical discharge re-heats the gas at the base of the projectile and sustains the base pressure for a longer time, thus increasing the velocity. Augmentation devices have succeeded in im proving the performance of a low-performance gun, but none have been applied as yet to a very high performance gun. Howell (Ref. 19) has reported that the velocity of a gun has been increased from 14,000 ft/sec to 22,000 ft/sec using this type of electrical augmentation.

Electrical energy can be added to the gas by arc discharge before or during compression. Reservoir temperature can be greatly increased and performance theoretically increased. Experiments are currently being conducted at the Naval Research Laboratory (Ref. 7) using this method of augmentation. No increase in performance has been observed to date; however, the experiments are continuing.

Traveling Charge Gun

The traveling charge gun (Ref. 20) is of interest because it is a low G accelerator and particularly well suited for launching fragile models. It can be described simply as being a gun boosted rocket in which part of or all of the propellant is affixed to the base of the projectile. Since it is essentially a rocket, it shares with the rocket the requirement for long acceleration distances if hypervelocities are to be attained. For successful operation, the burning rate of the propellant must be very high (about three orders of magnitude above solid grain burning rates) and well controlled. At present, these burning rates have not been achieved and no significant increase in muzzle velocity over conventional powder guns has been recorded.

Summary

In general, the adiabatic, isentropic compression guns are medium G accelerators. They can of course, be operated outside this range but their most successful application is in the medium G range. A reasonable freedom in the choice of projectile characteristics is available in the velocity range from 20,000 to 30,000 ft/sec.

Adiabatic non-isentropic compression guns must be classified as high G accelerators because of the short but high pressure pulse caused by the shock waves which are always present. Severe restrictions exist as to the type of projectile which can be launched from this type of gun although the velocity capability extends to 30,000 ft/sec.

Most of the non-adiabatic compression guns are high G accelerators. Although shock waves may not exist in the reservoir, extremely high pressures are applied to the projectile in most instances. The choice of projectile characteristics is quite limited. The steam-heated gun is an exception in that the acceleration produced can fall in the medium G range.

The characteristics of the combination guns are determined by the characteristics of its individual parts. The traveling charge gun is of interest in that it is the only low G gas gun with the theoretical capability of hypervelocity performance. Results of experiments with traveling charge guns have been discouraging, however.

SECTION IV: EXPLOSIVE DEVICES

Simple Explosive Designs

Three typical explosive charge designs used to accelerate hypervelocity projectiles are shown in Figure 6. In the first case, a pellet is placed on the end of a simple explosive charge and confined with a surround, which is usually made of a material such as lead, that will be vaporized by the detonation. The choice of explosive to be used in configurations of this sort is not particularly critical. In general, however, the explosives have been chosen both for their pressure and detonation velocity, together

with their characteristic handling properties. Typical explosives which have been used are: Pentolite (50% TNT/50% PETN), Composition B, and Composition C3. Each of these explosives has a detonation velocity of approximately 24,000 ft/sec. The second type explosive charge shown in Figure 1 is the "air cavity charge", wherein a pellet is imbedded in an air cavity in the base of an explosive charge. The third type uses a self-forging fragment imbedded in the end of an ordinary explosive charge. Charge designs A and C in Figure 1 have been made to accelerate masses to velocities approaching 22,000 ft/sec; however, the mass of the projected pellet has not been entirely reproducible. Consequently, they have not been used extensively to obtain terminal ballistic data. The most promising of the three charge designs shown is that using an air cavity, which permits some control of the pressure pulse which is applied to the projectile and thus affords a high degree of reproducibility of the mass and velocity of the pellet which is accelerated (Ref. 21). The basic operating principle of the air cavity and the purpose for the air cavity is to allow for the formation of a "Mach Disc" within the air cavity which acts to reduce the initial impulse to the rear of the projectile, and at the same time, to extend the duration of the applied pressure pulse. Thus, higher velocities can be achieved with charge design B (Figure 6) than would be possible with the conventional "end on" charge designs A & C.

By varying the dimensions of the air cavity and the thickness of the pellet, it has been possible to project intact masses having a known velocity and mass from velocities of 7000 ft/sec up to a maximum velocity of 22,000 ft/sec. In each case, the original pellet loses some mass around its periphery, the amount of material lost depending upon the particular charge design used. In general, it has been found that as the depth of the air cavity behind the pellet is increased, the velocity of the pellet is increased, up to the point at which the pellet breaks up entirely. As the velocity of the pellet increases, however, the mass lost around the periphery due to spallation increases. This mass lost is reproducible for a given pellet in a charge design. The final mass of a given pellet is reproducible to less than 3% of probable error. The total mass of the pellet accelerated is dependent upon the size of the explosive device used (since mass scaling laws hold for these charge designs), where masses of approximately 0.1 to 1.0 gram have been accelerated with a 1/8 pound explosive charge and masses to 7.0 grams have been accelerated with 11.0 lb explosive charges.

Shaped Charge Designs

Conventional shaped charge techniques have been used to accelerate particles to extremely high velocities (Ref. 22). A typical shaped charge using a copper liner produces a jet that is elongated in flight due to the velocity gradient from the tip of the jet to the tail. Thus, the continuous jet is extruded much like copper wire, and finally breaks up into discrete particles at a predetermined distance from the explosive charge. It has been possible, then, to consider the jet after it has broken up into discrete particles, as a series of individual elements and, by relation to the shaped charge theory, to determine the volume of the crater in the target due to each element of the jet length. Ideally, then from a single shaped charge firing, one can obtain the volume of the crater produced in a target per unit energy of the impacting material over a wide spectrum of velocities. Feldman, of the Ballistics Research Laboratory, used these techniques to obtain hypervelocity impact data for copper particles against a variety of target materials at velocities ranging from 26,000 ft/sec at the tip to approximately 7,000 ft/sec at the tail of the jet. An interesting adaptation of these techniques has been more recently exploited by Aerojet General Corp. in which an aluminum or titanium liner is used in the shaped charge (Ref. 23). Discrete jet particles could be obtained through a technique of detonating the explosive at a point off the axis of the charge. This causes a degree of malformation of a conventional shaped charge jet, yet does permit the acceleration of discrete fragments to velocities of approximately 27,000 ft/sec. The mass of each individual jet element, as well as its velocity, is then determined by multiview flash radiography just prior to impact of the elements on the target.

A ballistic projection technique capable of accelerating clusters of 10 to 200 micron sized particles to velocities of 50,000 ft/sec has been perfected using specially designed lined cavity charges (Ref. 24). Particle velocities of twice detonation velocity are possible with these charges (Ref. 25), with the total mass of projected particle cluster decreasing with increasing velocity. Brittle grey cast iron has been used as the shaped charge liner material which permits the acceleration of a cluster of microparticles having a reproducible size distribution and the density and properties of some natural meteorites. Two charges used to accelerate microparticles to hypervelocities are shown in Figure 7. In the first case, a 20 degree cone having a very thin wall (.010" or less) is used to obtain particle velocities up to 35,000 ft/sec. In the second case,

a 0 degree cone (cylinder) has been used to project particles up to 50,000 ft/sec. The charge designs and the properties of the materials are such that a conventional jet is not obtained from this type of shaped charge, but rather a cluster of particles is projected at high velocity, having a preferred particle size, depending upon the grain size of the original liner material. The small cluster of particles, having little or no velocity gradient is accelerated and is trailed in flight by the slug and residue material, which are a natural consequence of any shaped charge device. This massive slug material is moving at a low enough velocity so that it is possible to use an explosive actuated cut-off device to prevent the impact of this debris on the target of interest. The size distribution of the cluster of microparticles is determined by recovering the particles in paraffin, then determining the distribution of particle sizes by actual count under a microscope. This size distribution of microparticles is then related to the size distribution of craters produced on a target specimen. The relation of the particle size distribution to the crater size distribution on a target then defines the size of the crater produced by a given particle. These techniques have been successfully exploited in the study of the microparticle cratering phenomena and the scouring of a target surface by micrometeorites. A typical distribution of the particles found in the cluster and of the craters produced on impact is shown in Figure 8.

Using special techniques and cylindrical liners, it has been found possible to accelerate short rod-like projectiles to very high velocities. A velocity of 60,000 ft/sec has been obtained (Ref. 1) velocities of 90,000 ft/sec seem feasible.

SECTION V: SUMMARY AND CONCLUSIONS

The capabilities of various types of accelerators have been discussed. No single device is superior in all respects. However, for various classes of projectiles, certain devices are better than others. For projectiles whose size, shape, and composition must be precisely controlled, the light-gas gun, and in particular, the accelerated-reservoir light-gas gun, seems superior. This gun has a proven capability for producing projectile velocities in excess of 32,000 ft/sec and has a potential for

significant future development. By pre-heating the hydrogen before compression in this gun, the velocity capability should reach 40,000 ft/sec.

Combination guns are of interest because they offer the potential of improving the performance of the best single type gun. Historically, however, development of single type guns has kept ahead of all efforts to produce a better combination gun. This trend will probably continue since the development of a combination gun is a more complex problem than the improvement of a single type gun. In principle, however, a combination or augmented gun will eventually prove superior so efforts in this direction should continue.

For accelerating projectiles composed of a ductile metal with reasonable, but not precise control of shape and size at velocities up to 25,000 ft/sec, explosive devices are attractive because of their simplicity and low cost per shot.

Microparticles have been accelerated to 45,000 ft/sec using explosive devices, and a potential exists of increasing this velocity to 60,000 ft/sec or more. Electrostatic accelerators have fired submicron sized particles at 32,000 ft/sec and a potential exists for a substantial increase in this velocity. However, the simplicity and ease of operation of explosive devices may make them more attractive for microparticle acceleration.

Short jet shaped charge devices have the current capability of firing projectiles at a velocity of 60,000 ft/sec and have a potential of exceeding 90,000 ft/sec and have a potential of exceeding 90,000 ft/sec.

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	CURRENT		POTENTIAL	
	Mass (GM)	Velocity (ft/sec)	Mass (GM)	Velocity (ft/sec)
Microparticles	10^{-11} to 10^{-5}	26,000 to 50,000	10^{-12} to 10^{-4}	23,000 to 65,000
Simple Shapes	10^{-2} to 10	6,000 to 23,000	10^{-2} to 100	6,000 to 26,000
Short Jets	10^{-1} to 10	26,000 to 65,000	10^{-2} to 100	23,000 to 100,000

Table 1. Explosive Simulation of Meteors

From ARS Paper No. 2030-61. (Ref. 1)

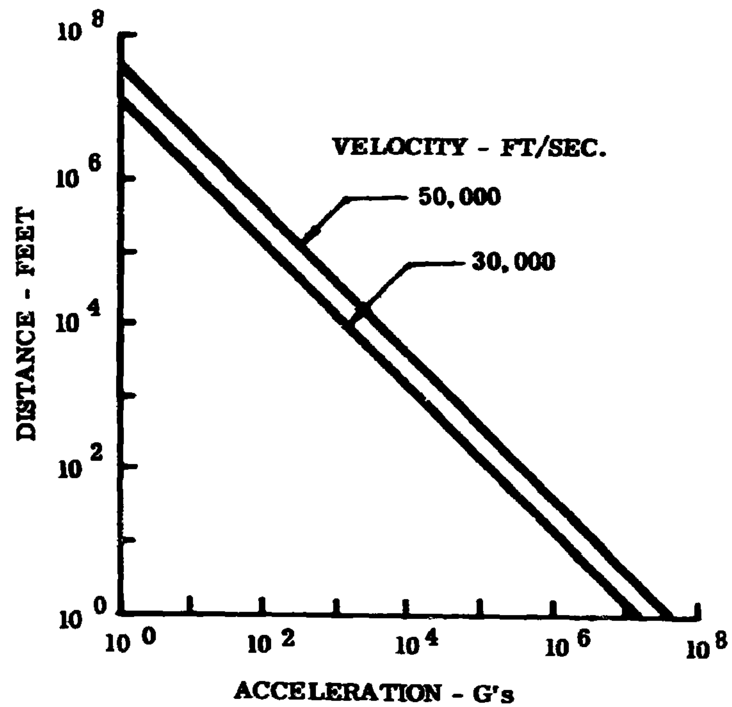

Figure 1. Distance Required to Achieve Velocity With Constant Acceleration

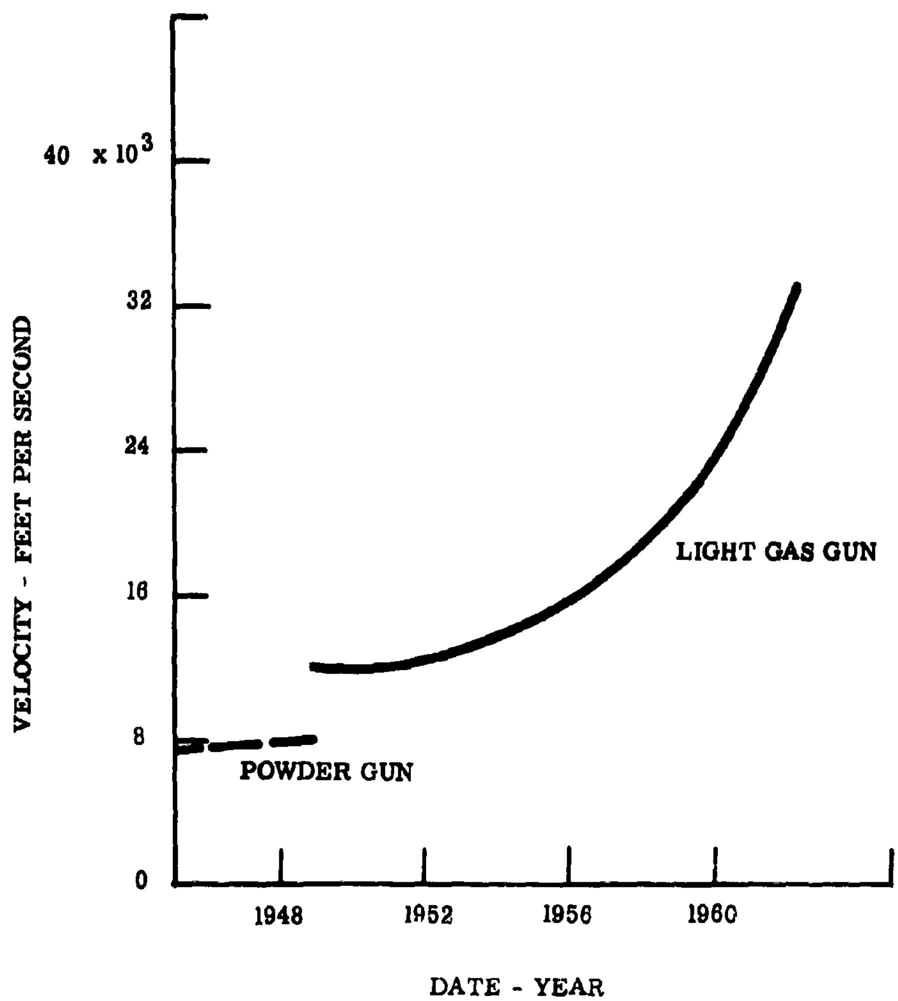

Figure 2. Maximum Gun Velocities

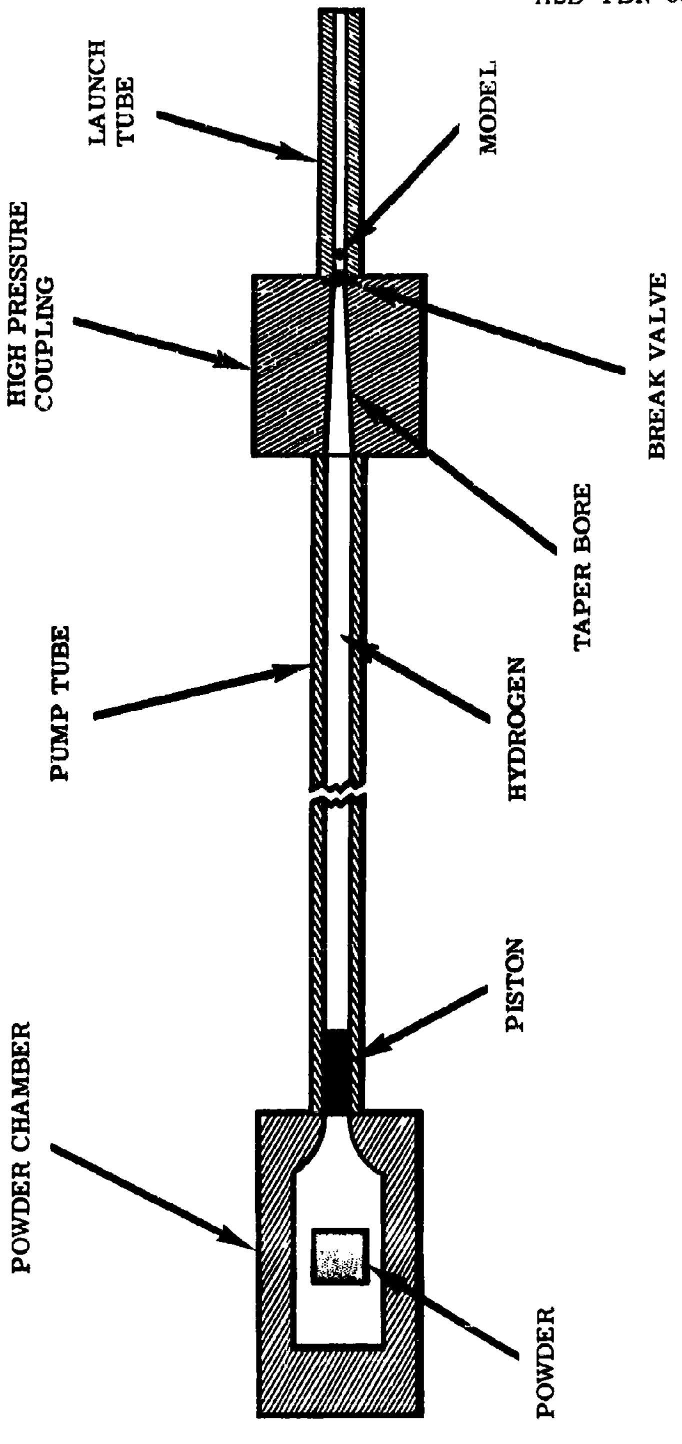

Figure 3. Adiabatic - Isentropic Compression Light Gas Gun

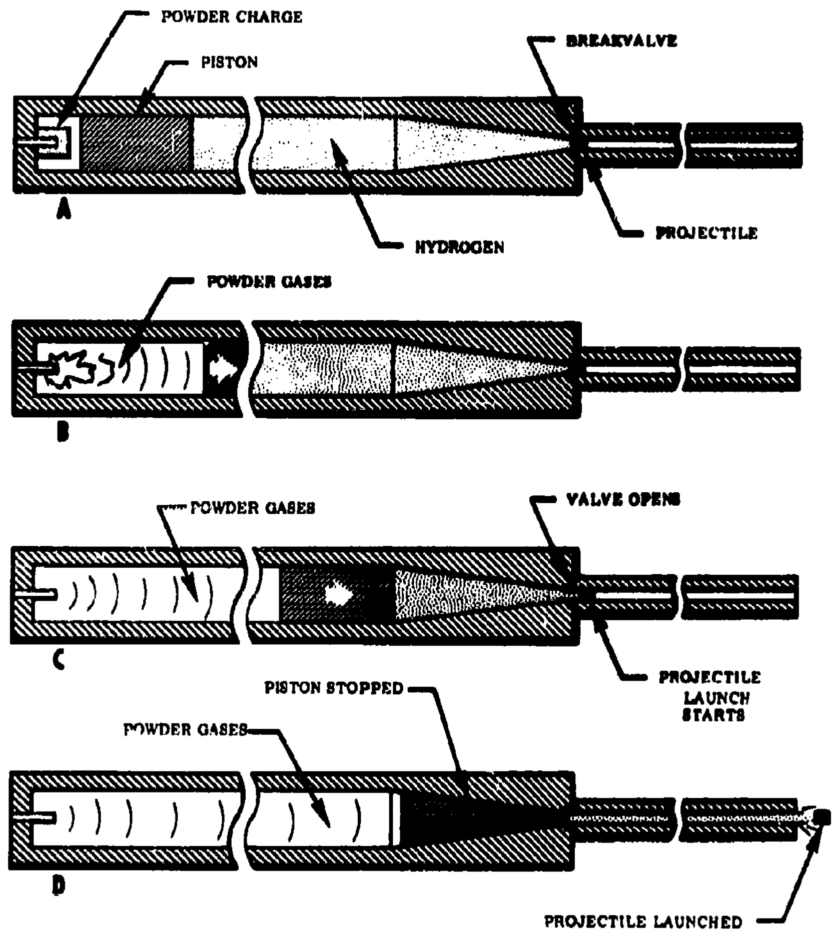

Figure 4. **Accelerated Reservoir Light-Gas Gun Operating Sequence**

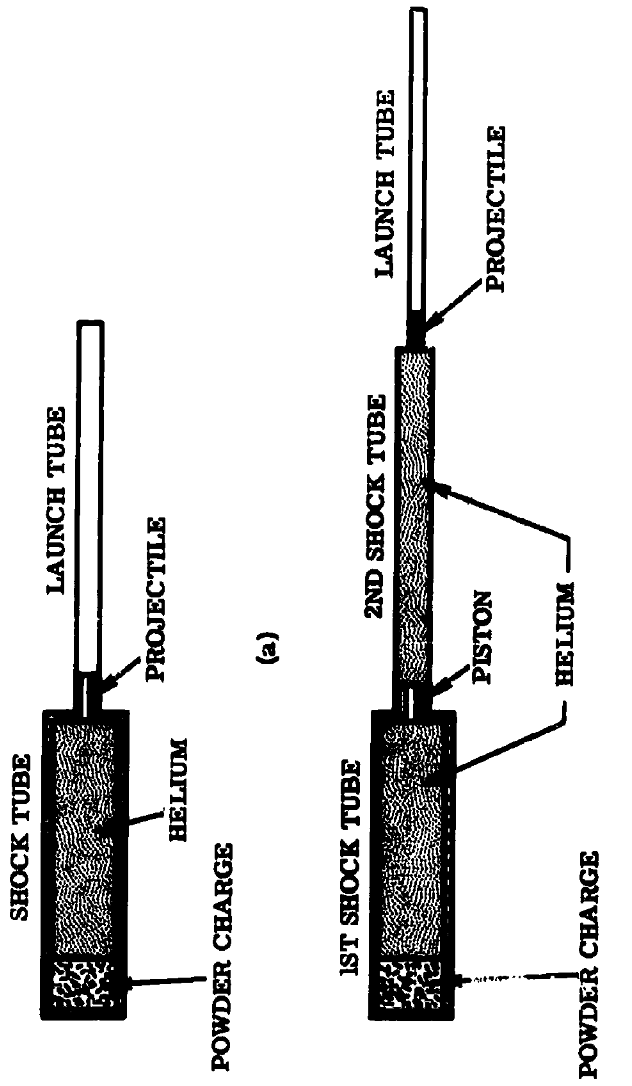

Figure 5. Shock Compression Light Gas Guns

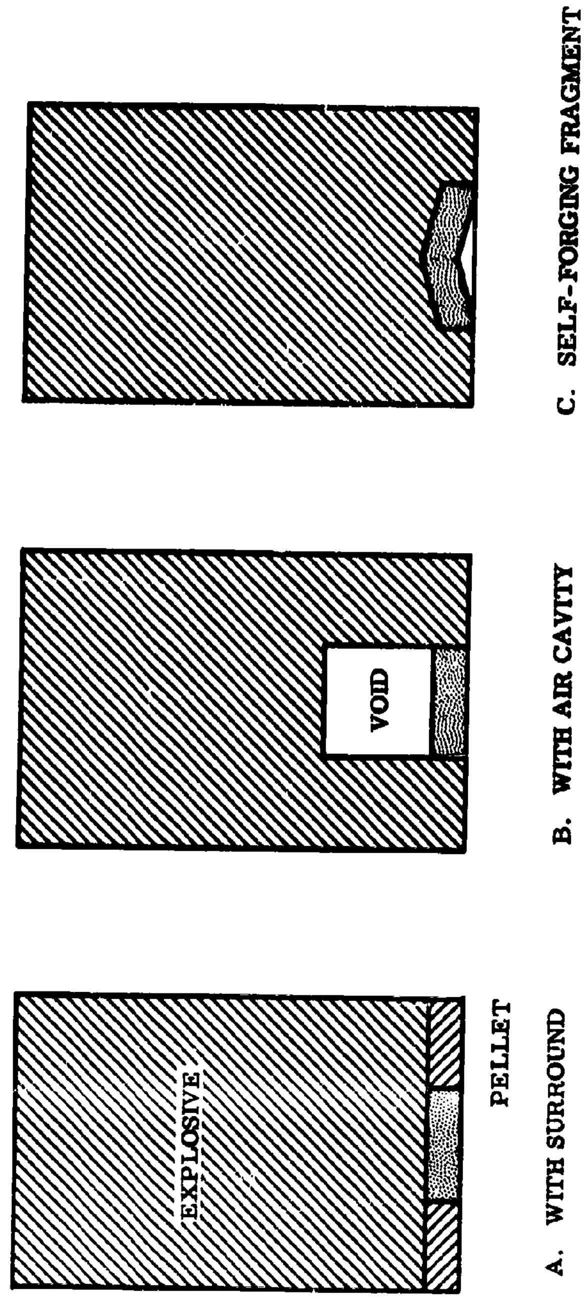

Figure 6. Simple Explosive Charge Designs

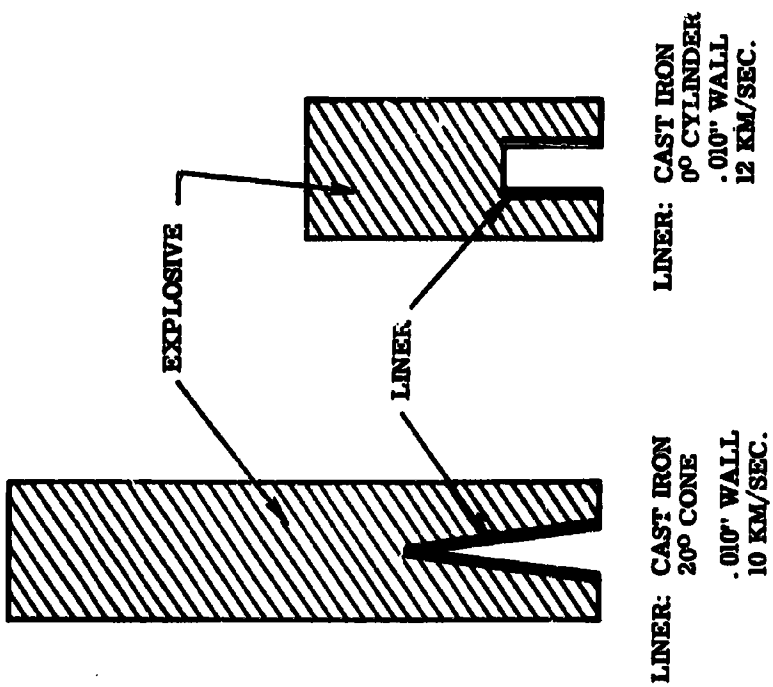

Figure 7. Charge Designs for Microparticle Acceleration

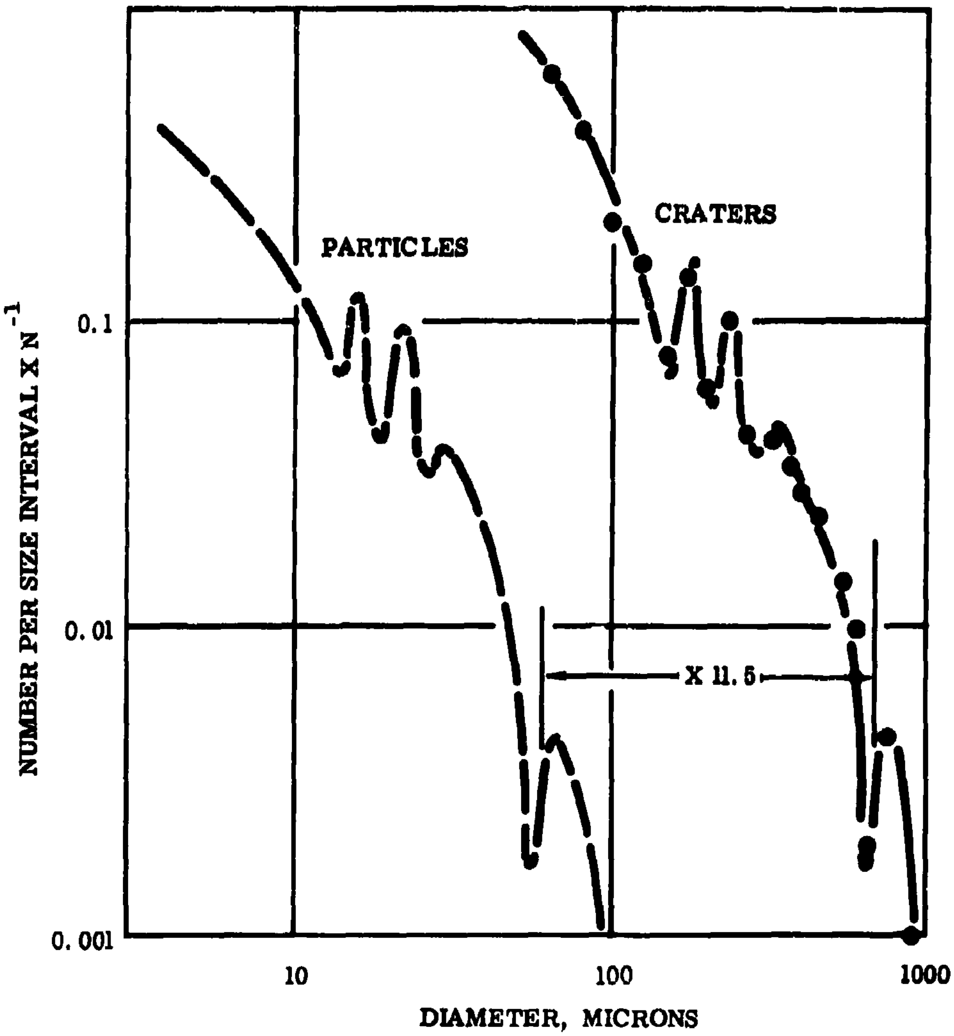

Figure 8. Typical Size Distribution of Microparticles and of Craters

DISCUSSION

DR. BISPLINGHOFF

Thank you very much, Mr. Curtis. Are there any nonexplosive questions in the audience?

COLONEL STANDIFER

There is a project on accelerated particles, joint in-house andcontract effort. I'd like to ask our project engineer if we have some potential velocities a little better for smaller particles, possibly.

HOPKINS

Since you deviated from your original premise of knowing the composition, geometry, and velocity of the projectile, I think there is a rather significant breakthrough in accelerating projectiles that we can point out here. We have a contract in being right now with Technical Operations, Inc., in which they are accelerating small projectiles using the exploding wire technique. We are talking about projectile sizes--ten milligram plastic projectile, one-quarter inch diameter by ten mils thickness, with 25 to 1 d/L. Our contractor has been successful in accelerating these projectiles routinely to above 50,000 feet per second on a slow discharge capacitor system; several shots accelerated to above 60,000 feet per second. On a fast discharge system, he has been successful in accelerating projectiles to above 100,000 feet per second, a real significant breakthrough. However, there are difficulties involved with this particular system in that we do not know for certain the projectile shape at the point of impact. We have no techniques for measuring a small diameter, small thickness projectile in flight. We will know the mass of the projectile after construction of the ballistic pendulum, conical type. We are continuing this development work. Right now, NASA, Langley, has a contract with Technical Operations in which they are accelerating spherical projectiles using a flight disk as a sabot. We are talking about a maximum to date of 64th-inch copper spheres being accelerated to velocities on the order of 50,000 feet per second. This is a .3 milligram sphere. The sabot protects the projectile from ablation during flights so we have a fairly good idea of what the projectile size is at impact. I think that this information should be brought out now.

MR. CURTIS

My congratulations to Technical Operations. I was unaware of this development.

DENTON, ARMOUR RESEARCH FOUNDATION

You had mentioned that you thought the electrical discharge augmentation was limited to rather poor performance guns. Do you feel there is some inherent reason for this limitation?

MR. CURTIS

No. I think that the people who have been experimenting with it have chosen a gun whose performance is well known. It takes time to develop the instruments, and the velocity of guns has been increasing so rapidly, that I just don't think they have been able to pick the best gun and keep up with it. I don't think there is anything technically wrong with the approach. In fact, logically, it seems that this should be the best gun eventually. It's a little difficult to keep up with the advancing state-of-the-art when you have such a steep rising curve of performance versus time.

SALISBURY, ASD

In noticing all the devices with the exception, let's say, of the improved shaped charge projector or our own work at Tech Ops, it is amazing how much money and effort has been expended in a range where you are just transitioning from a low conventional ballistic impact to the place where you have fragmentation of the projectile and the beginning of a curve of hypervelocity impact. You are just barely scratching that low end of the meteoroid velocity range. I just wonder how much it is worth to make these incremental changes in these devices as opposed to an exotic approach, insofar as trying to control geometry is concerned. If you can make a number of runs where you don't control geometry, but diagnostically, you can measure the projectile, its size and shape and everything else, then statistically, you can still come up with data just as valid as if you controled projectile geometries. Also in many of the gas guns your projectiles are not similar either in size or geometry to what probably would be expected of 90 or 95 percent of the meteoroids in free space.

MR. CURTIS

What you say is true. Light gas guns, however, have a rather dual purpose. This organization is particularly interested in structural dynamics in impact. There are those who are interested in aerodynamics and physics of flight, and for these, the light gas gun is indispensable; this application alone will justify their development. As to the statistical determination of damage from explosively launched projectiles, this again is probably satisfactory for a large part of the structural dynamic studies. However, for countermeasure studies, for kill mechanisms, there you have control; you know what the projectile is going to hit and you want to be able to assess the damage on the real thing. There, again, I think that these applications will justify the light gas gun. The incremental increases in velocity are agonizing, I agree with you, but they are apparently worth the money since they are becoming increasingly more popular.

DR. BISPLINGHOFF

Any other questions? Any more comments? Any more discussion? If not I suggest we adjourn until 9:00 o'clock in the morning.

TECHNICAL SESSION IV

FRACTURE PHENOMENA

H. J. Plass, Ph.D.
Session Chairman

University of Texas

TECHNICAL SESSION IV

INTRODUCTORY REMARKS

COLONEL L. R. STANDIFER

This morning we are taking up our final session on fracture. It gives me great pleasure to introduce Dr. Harold Plass from the University of Texas. He received his B.S. and M.S. degrees at Wisconsin and his Ph.D. degree at Stanford University. He has been particularly active in the Society of Engineering Education and the Society of Mechanical Engineering. He started out originally as a Research Engineer with the Defense Research Labs and is now Associate Professor of Engineering Mechanics at Texas. He has been there since 1954. Without further ado I would like to welcome Dr. Plass.

DR. H. J. PLASS

Chairman, Session IV

Mr. Chairman, lady and gentlemen. Yesterday the presentations that we heard, particularly in the morning and afternoon sessions, were concerned more with the details of the properties of material, and less with the mechanics, although there was some of the mechanics of the material involved in several of these papers. Also the papers were concerned with the behavior under very intense loading and loading which changed rapidly with time. These intense loadings in general were compressive type. Today the papers are concerned more with the behavior of materials under high intensity tensile loads where the intensity of the load is high enough to cause separation of parts of the material, that is, to cause a fracture in the material.

Other types of stress fields besides the tension field are also discussed; however, the main attention is paid to the tension field. In the past the criteria for fracture were considerably oversimplified. Engineers were attempting to prevent the occurrence of fracture by designing the material in such a way that the stress state in the material never exceeded a certain prescribed maximum. In other words it was believed for many years that fracture was simply an occurrence which took place when the state of stress reached a certain value in the material, and then, bang, it fractured. Recently, researchers have shown that fracture isn't that simple, that it actually is a process which requires a certain amount of time, and that during the process of fracture the details of the state of stress and strain and the rate of stress and strain in the vicinity of the separating parts of the material are important.

Today we will hear three papers concerning the fracture phenomena. The first is titled "Fracture by Progressive Crack Extension" and will be presented by Dr. George Irwin of the U. S. Naval Research Laboratory. He is at present the Superintendent of the Mechanics Division at this Laboratory. He attended Knox College where he received a Bachelor's degree in 1930. From the University of Illinois he received a Master's degree in 1933 and his Doctor's degree in 1937. He is a member of the American Physical Society, American Society of Testing Materials, Washington Philosophical Society, Washington Academy of Science and the Research Society of America. He has published many papers in the area in which he will be reporting this morning. Without further comment, I will turn the meeting over to Dr. Irwin.

DR IRWIN

Thank you Dr. Plass. It is a pleasure to add one more contribution to this symposium with its wide range of topics, and I suppose that we are all specialists and we all add our bit to the breadth of topics discussed in the line of our specialties. Now as you know, structural grade solid materials contain defects which reduce the strength--the fracture strength--some orders of magnitude below that which would theoretically be present in the absence of such flaws. Consider what happens when something breaks. As an increasing tensile load is applied to a component throughout the region subjected to largest tension there will be deformation near these flaws or defects.

FRACTURE BY PROGRESSIVE CRACK EXTENSION

by

George R. Irwin, Ph.D.

U. S. Naval Research Laboratory

FRACTURE BY PROGRESSIVE CRACK EXTENSION

George R. Irwin

U. S. Naval Research Laboratory

ABSTRACT

This paper discusses the conditions governing the development of unstable progressive crack extension at normal speeds of stress application. Progress in this field has resulted from separating the topics of fracture initiation and crack extension. The relevance of laboratory tests to service behavior may then be represented for fracture initiation by flaw statistical analysis and for crack extension by fracture mechanics analysis. The concepts now at hand for fracture strength and for the ductile-brittle fracture mode transition will substantially assist the establishment of sound relationships between atomic and macroscopic properties. The impact of new viewpoints is expected to be of special practical interest in application to new fields such as composite materials, adhesive joints, and fatigue.

FRACTURE BY PROGRESSIVE CRACK EXTENSION

Most structural grade solid materials contain defects which reduce the fracture strength several orders of magnitude below what might, theoretically, exist in their absence. As an increasing load is applied to a component, throughout the region subjected to largest tension deformations near the defects produce tiny openings which join so as to produce eventually one or more starting cracks. Propagation of these cracks produces the observed final separation. Because the flaw or small-crack growth rate tends to be a rapidly increasing function of flaw size the race between different defect locations for the honor of causing failure of the piece is a highly biased competition. The big ones grow bigger not only in proportion to their own size but also in proportion to the number of nearby smaller defects with which merger can be effected. Thus there is rarely more than one winner of the competition and a fracture produced by moderate loading speed normally has only one primary origin.

At the opposite extreme of very rapid loading, say by explosion forces, competitive flaw growth is suppressed, and the multiplicity of independent origins may result in a separation showing evidence of progressive crack extension only at microscopic scale. The present paper is concerned primarily with fractures of the "one origin" kind and with the resistance to progressive crack extension. Table I lists classes of analytical tools and general types of experimental observation used to obtain knowledge and control of fracture. These lists range from macroscopic to atomic scale. The lower entries in the table possess more pure science appeal and apply to a much broader range of subjects than just fracture. The practical usefulness of these in relation to fracture lies mostly in the future. It is, at any rate, encouraging to note the increasing trend to replace critical stress concepts, where possible, by the mechanistic viewpoint of crack toughness.

Confining attention to practical utility, Table II lists current applications served by the activities given in Table I. For effectiveness in serving these as well as other needs and to provide a sound technical understanding none of the tools which can assist this understanding should be neglected. Each tool is one component of an overall pattern or synthesis. Although aspects which are of basic importance to interrelating components of the overall pattern remain in considerable doubt, nevertheless the general outline of the pattern has become clear and an understanding of this general outline isdesirable for guidance of future work.

STEADY FLOW MODEL

Consider first the schematic representation of progressive crack extension shown in Figure 1. Near the crack border there is a zone of inelastic strains which we will term the process zone. This zone is represented by the circle. The average time rate of movement of the crack border, as progressive separation occurs, is represented by giving the material particles a velocity c in the right-to-left direction. The phenomena occurring can then be discussed in terms of a coordinate system (r, θ), the origin of which remains at the center of the process zone as the crack extends. If all of the controlling parameters (temperature, chemical environment, material properties, and stresses) in the material outside the process zone are held constant, then c is constant and the time average flow pattern corresponds to a "steady flow" condition.

Consider the right-to-left journey of a small volume of material which spans the x-axis. Starting from the right side of the process zone the inelastic strains increase with leftward motion. As the element nears the crack border these strains have resulted in small separations which merge together and with the main crack as the element contacts the left side of the process zone. When one states the problem in these simple terms it might seem that much could be done toward explaining crack extension in a theoretical manner. In a relative manner of speaking this is true. However, analytical representation of the entire progressive separation process is quite beyond us at this time and will require, as noted above, careful use of all of the analytical tools.

It is the task of linear-elastic fracture mechanics to provide a suitable characterization of the stress field surrounding the process zone and the relationship of this crack border stress field to crack length, specimen shape, and applied loads. It is the task of plasticity mechanics to describe macroscopic aspects of the inelastic strains within the process zone. Using the elastic and plastic analyses for guidance, in crystalline materials which deform by slip, dislocation mechanics should provide the mechanisms of formation of micro-cracks and cavities. The analysis tool called flaw-statistics assists representation of the influence of defects with enough generality and versatility to be of value either alone or in support of all the other analysis tools.

Sound increments of work have been contributed by all of the analysis tools of Table I with the possible exception of A6. The two aspects most in need of work are:

(a) the development of closer relationships linking together the various phenomena at different size levels and

(b) clarification of the ways in which a statistical mechanics approach can be used to determine the response, c , as a function of the driving force and of the other parameters which govern the crack extension process.

THE ELASTIC STRESS FIELD

The simplest representation of the elastic stresses surrounding the process zone is obtained from the linear elastic approximation of the crack stress field. In this viewpoint the crack is assumed to be a flat internal free surface bordered by a curve of simple shape. The stress relaxing influence of inelastic strains is neglected except for an adjustment which locates the crack border of the assumed linear-elastic crack at a central position within the process zone. The elastic stresses can then be stated in terms of an appropriate linear combination of the crack border stress equations for the three crack surface displacement series. The three modes are:(1)

	Name	Crack Surface Displacement
Mode I	Opening Mode	Normal to the crack surface
Mode II	Forward Shear Mode	In the crack surface normal to the crack border
Mode III	Parallel Shear Mode	In the crack surface parallel to the crack border

A compact summary of these equations is given in reference (1). Here it will be sufficient to look at the equation for σ_y in the Mode I system.

$$\sigma_y = \frac{K_I}{\sqrt{2\pi r}} \cos\frac{\theta}{2}\left(1 + \sin\frac{\theta}{2}\sin\frac{3\theta}{2}\right) \text{ - - - - - - - - - - - - } (1)$$

Where r and θ are shown in Figure 1.

The opening mode crack border stress system contains, in addition to K_I , three other parameters which do not appear in equation (1) because they have no influence upon σ_y . However these three merely represent unifrom stress fields which can be added to σ_x, σ_z, and τ_{xz}. The last of these uniform added stresses one expects to be eliminated by the crack border orientation; none of them have a bearing on the stress

singularity, and experiments suggest they have negligible direct influence upon crack extension speed.

From consideration of this kind each of the three sets of stress equations can be given a one-parameter characterization in terms of the stress intensity factors K_I, K_{II}, and K_{III}. The influences of crack length, specimen shape, and applied loads are represented through their effects upon the K values.

As noted above, it is necessary to select a location within the plastic zone for the origin of the r, θ coordinate system. This origin is regarded as placed so that the linear elastic stress field has a "best fit" relationship to the actual stress field in regions away from the zone of non-linear strains. Actually, there is no unique "best-fit" position in the case of real cracks. For the mathematically simple Mode III shear crack discussed by McClintock[2], the plastic zone has the shape and position shown in Figure 2 and the origin would be placed at the center of the circle. Various rules have been suggested for fixing the position of the linear-elastic crack border relative to the real-crack border in the case of tensile fracturing.[3] [4] These appear to serve well enough for practical uses of the linear-elastic approximation. The suggested small adjustment of the origin away from the real crack border has a magnitude r_Y of about 0.1 to 0.2 times $(K/\sigma_{YS})^2$ where σ_{YS} is the uniaxial tensile yield stress. The generalized force (conjugate to time rate of crack extension) is the elastic energy flux into the process zone per unit area of crack extension. The value of K_I is related to the crack-extension force component $\mathcal{G}_I$ by the equation

$$E\,\mathcal{G}_I = (1 - \nu^2)\,K_I^2 \quad \text{- - - - - - - - - - - - - - -} (2)$$

where E is Young's Modulus and ν is Poisson's ratio. Similar relations hold for the crack-extension forces $\mathcal{G}_{II}$ and $\mathcal{G}_{III}$ in terms (respectively) of K_{II} and K_{III}.

The symbol $\mathcal{G}$ was selected in recognition of the fact that it was the theory of fracture strength of A. A. Griffith[5] which first focused attention upon the importance of the rate of transfer of energy from the stress field to the separation process. In his best known contribution to this area of work Griffith proposed that equating twice the solid state surface energy to $\mathcal{G}_I$ would permit theoretical calculation of fracture strength in terms of some basic flaw size in the material.

The Griffith theory received renewed interest from proposals by Irwin[6] and Orowan[7] that a near correspondence to various observations was obtained if the surface energy factor was replaced by plastic work

per unit fracture area. This, in turn, led to the force concept and to the development of a mechanics of fracturing based upon observations of the force-response relationship. Thus the current viewpoint is primarily descriptive rather than theoretical. Both in theoretical and in descriptive applications the steady flow model is the basic reference point.

CRITICAL FRACTURE STRESS

In order to establish the connection between laboratory strength tests and strength in service applications so that a relationship to process details is preserved one of two paths must be followed. The first path, usually preferable, can be followed when a clear statement can be made of the failure process operative during development of a service fracture. The strength limiting aspects of this mechanism can then be represented in laboratory or model tests. For example, the failure mechanism may be crack extension assisted by fatigue, by environment, or by timewise deterioration of the material. Laboratory studies of crack extension along lines previously indicated are then applicable. The second path is the remaining alternative when, to single out a particular strength limiting feature of a service failure, is regarded as impractical. In this event it is possible and desirable to preserve a degree of attachment to failure mechanism ideas through use of flaw-statistical analysis methods.

CRITICAL STRESS FOR BRITTLE FRACTURE

Flaw-statistical analysis methods are discussed elsewhere(8) and this paper is primarily concerned with treatments which assume a specific failure mechanism. Ideally the viewpoints are complementary, Note, for example, that the flaw statistical analysis requires as a basis a limiting failure stress concept. In order to assist the above complementary relationship it is helpful to use a limiting failure stress concept which is consistant in some plausible way with individual failure process mechanisms. The modified Griffith-crack theory is useful for such a purpose and this is illustrated in the following discussion.

Consider the composite material is isotropic and contains randomly oriented flaws each of which can be regarded as equivalent to a small crack. The treatment assumes each flaw is a plane two-dimensional crack of half length a. However, equivalent results would be obtained assuming the crack to have a circular shape. It is assumed crack extension occurs either in tension by Mode I or in shear by Mode II, the resistances to crack extension being $\mathscr{G}_{Ic}$ and $\mathscr{G}_{IIc}$(or, alternatively, K_{Ic} and K_{IIc}). In a statistical treatment the chance of flaws of various degrees of seriousness as governed by the a-value and by orientation would be considered. Here, however, we confine the analysis to a discussion of what failure stress would be expected if the a-value is invarient and if the local frequency of flaws is high enough so that at least one crack has the most favorable orientation.

The stress on the region under consideration will be assumed to be such that the principal extension stresses are σ_1, σ_2, σ_3.

The failure criteria for the two modes of crack extension are given by

$$\sigma \sqrt{\pi a} \geq K_{Ic} \quad (\sigma \leq 0) \qquad (3)$$

and

$$(\tau + f\sigma) \sqrt{\pi a} \geq K_{IIc} \quad (\sigma \leq 0) \qquad (4)$$

where σ is the tension normal to the plane of the crack, τ is the magnitude of the shear stress resolved parallel to the crack and normal to the leading edge, and f is a coefficient of friction. Equation (3) is the usual Griffith relationship. The form of equation (4) was suggested by McClintock and Walsh[9]. Discussions of this idea in connection with strength of rocks have been given by Orowan[10] and by Brace[11]. When σ is negative the action of the shear stress τ for causing Mode II crack extension is opposed by the friction stress f_σ. The effect of the friction is equivalent to decreasing τ by the amount $|f\sigma|$ in the calculations of $\mathcal{G}_{II}$ and K_{II}.

Consider that $\sigma_1 > \sigma_3$ and $\sigma_2 = 0$. If σ_3 is positive fracture by Mode I is predicted and the failure locus from equation (3) is a fixed limiting value of σ_1 say σ_c. However, when $\sigma_1 - \sigma_3$ becomes large enough, through decrease of σ_3 into the compressive range, then shear failure by Mode II is expected in accordance with equation (4).

If we put

$$\alpha = (K_{IIc}/K_{Ic}$$

equations (3) and (4) may be written

$$\sigma \geq \sigma_c \qquad (5)$$

and

$$\tau + f\sigma \geq \alpha \sigma_c \qquad (6)$$

Equation (6) dominates when $(\tau + f_\sigma)$ is greater than $\alpha\sigma$. The failure locus in a σ_1, σ_3 plane of the principal stress values assuming $\alpha = 2$ is shown in Figure 2. The stress values on the figure are in units of σ_c, and f was assigned the value 0.75. Along ABCD Mode I

fracturing occurs on a plane normal to the greatest tension. Along DEF Mode II fracturing occurs on a plane at 26.7 degrees to the σ_3 or y-direction. Generalization of the result for values of σ_2 other than zero is not difficult.

A diagram like Figure 2 might be suitable for certain rocks where the compressive strength is judged to be an order of magnitude greater than the tensile strength. For grey case iron the choices $\alpha = 1.3$ and $f = 1/4$ closely represent the observations by Grassi and Comet.[12]

CRACK PROPAGATION TOUGHNESS

Observations of the force-response relationship for crack extension in various materials shows that onset of rapid fracture usually occurs in a relatively abrupt manner. In other words the time rate of crack extension can be increased several orders of magnitude by a change of 10 percent or less in the value of $\mathscr{G}$ if the value of $\mathscr{G}$ is close to a critical value which is termed $\mathscr{G}_c$. Both the concept and the measurement procedure for $\mathscr{G}_c$ (or, equivalently, K_c) have much in common with corresponding aspects of the tensile yield stress σ_{YS}. With both questions arise regarding the degree of abruptness and how to define the property in terms of measurement techniques.

In the case of the yield stress concept lack of settlement of these questions to everyone's satisfaction has not prevented collection results and their useful employment during the past 100 years. The technical history of σ_{YS} measurements should be required reading for those who plan to await settlement of all uncertainties prior to engaging in measurements of the $\mathscr{G}_c$ (or K_c) type.

Crack toughness measurements are relatively simple for materials with only a small sensitivity of plastic flow stress to strain rate such as the high strength steel, aluminum, and titanium alloys. Primarily one needs a test with a notch of extreme sharpness in a specimen size large enough so that the process zone can be considered enclosed by the elastic stress field. A continued effort toward test standardization will be helpful. However, measurements can be made of plane stress K_c values using notched sheet tensile specimens and of opening mode plane strain K_{Ic} values using a variety of specimens with a useful degree of accuracy on the basis of available publications.[13][14][15][4]

FRACTURE MODE TRANSITION

In the field of metallurgy the impact of fracture mechanics is easily seen in the concept of a brittle ductile transition temperature. From the fracture mechanics viewpoint a tensile fracture can change from

a tough oblique shear separation to a brittle transverse separation simply by increase of the dimension of the test piece parallel to the crack border. Mid-range conditions exist when the crack border plastic zone size ($\sim 2r_Y$) is approximately equal to the plate thickness. The effect is, of course, due to the influence of elastic constraint on the size of the plastic zone.(16)

In order to insure a substantial degree of toughness, enough, for example, so that a through-crack of twice the plate thickness, B, is stable when the tensile stress equals σ_{Ys} the condition is $r_Y = B$(13) (4) or

$$K_c^2 = 2\pi\sigma_{Ys}^2 B \text{ - - - - - - - - - - - - - - - - (6)}$$

Since the fracture appearance depends upon the degree of elastic constraint acting at the plastic zone, the control of brittleness through the transition temperature concept has much in common with the short through-crack toughness criterion noted above. What has been added through fracture mechanics analysis is primarily a more precise understanding in terms of the fracture failure process and an appreciation for the important role of section dimensions.

THE STEADY FLOW MODEL APPLIED TO STRESS CORROSION

Essentially the basic experimental task of fracture mechanics consists in collecting observations of crack-extension response to the crack-extension force $\mathscr{G}$. The degree of confidence with which analytic interpretations can be made is clearly optimum when the experiment provides steady state or pseudo-equilibrium conditions in which $\mathscr{G}$ and the opposing equal resistance to crack extension are constant and pertain to some constant speed of crack extension.

In measurements of crack toughness of those high strength metals which posses relatively small dynamic elevation of yield strength, observations of crack-extension force are made at the point of onset of rapid fracture with the crack speed accelerating. In such applications lack of control of crack speed at the measurement point is reasonably considered of minor importance.

However, for materials with pronounced strain rate sensitive yield properties or for progressive stable crack extension due to influences such as stress corrosion, fatigue, or corrosion fatigue, observations under conditions of known, constant, or slowly changing crack speed are of special value. As a sample illustration of this type of work we consider crack extension under stress corrosion conditions.

To assist a centering of interest upon the factors of major importance one may employ a "steady flow" model as illustrated by Figure 1. Within the region adjacent to the crack border the applied loads produce plastic strain and new fracture origins. The chemical environment has access to the crack border through the crack opening and assists the development and joining of fracture origins so that the crack border advances into the metallic solid at the average time rate c. By moving the origin of position coordinates to the right with the velocity a the average position of the crack border in terms of the moving coordinate system would remain fixed. Granting a moving coordinate system of this kind has been established, it is evident that a time-independent flow condition corresponding to a constant value of c should be observed so long as the stress environment, temperature, and chemical environment are held fixed.

Experimental results from work by H. H. Johnson[17] have demonstrated the expected behavior occurs. A selected set of observations is shown in Figure 3. The specimen was a flat sheet of a hot die steel (H-11) at a yield strength of 230,000 psi having a central transverse slot with sharply notched ends. Helium gas saturated with water vapor was circulated in a chamber enclosing the central portion of the specimen. Increasing increments of tensile load were applied with a holding time of 5 minutes at each load value until initial cracks at the notches were observed. Thereafter the load was held steady except for readjustment to maintain a constant value of the crack-extension force $\mathcal{G}$ (or K). The values of crack length as a function of time shown in Figure 3 were inferred from electrical resistance measurements based upon a previous calibration. With the aid of graphs it was possible to readjust the tensile load to maintain K approximately constant.

After the crack length exceeded W/2 both the prior calibration and equation used to calculate K lost accuracy and the crack extension speed began to deviate from a constant value.

The influence of various K-values upon the crack extension speed is shown in Figure 4. These results as well as others, not shown, at different relative humidities suggest a K of about 18 ksi $\sqrt{\text{in.}}$ was necessary before the environment became effective in producing continued crack extension.

In the test specimen used by H. H. Johnson a small amount of plane-stress plastic yielding where the crack border meets the specimen surfaces provides an uncertain fraction of the stability against rapid crack extension. In addition a uniform environment influence along the entire crack border may not be present. Although the extension of a long through-crack as in the above experiment may not be a close model of real crack in structures, there are compensating advantages from the viewpoint of controls. For example, the environment at the crack border,

even though somewhat complex, can be held constant independent of crack length. Studies of the general nature of the above experiment might be done with various materials, environments, temperatures, and stress states as an aid to better understanding of the mechanisms responsible when crack extension is assisted by corrosion or by environmental influences of other kinds.(18)

SEPARATION OF ADHESIVE JOINTS

A structural composite material might be defined as a set of close fitting elements connected by adhesive joints. A minimum of voids and strong adhesion is necessary for a high quality tungsten carbide tool. For a glass fiber laminate used as fragment armor, interlaminar and interfiber separation under impact is, to a certain degree, desirable as this action blocks development of crack propagation across the fibers and is needed for optimum toughness. Clearly adhesive joints are important in many different ways. In addition there is a strong interest in the subject of adhesion on the part of scholars. Mechanical failures of adhesive joints rarely occur otherwise than by progressive separation. Thus the field is a natural area as well as a rich one for applications of fracture mechanics methods of analysis. Because of inherent complexities and the fact that the first work in this field is so recent the results currently available are exploratory and may not be closely indicative of future developments.

If we consider a crack spreading in a large block of isotropic material, the crack plane is essentially normal to the greatest tension except under a stress state dominated by large amounts of compression. A Mode II separation is seldom observed. However, when an adhesive joint constitutes the weakest element linking two components the locus and direction of the failure process remain in the adhesive joint under a wide range of stress states. Thus critical $\mathcal{G}_{II}$ values are readily measured for a flat adhesive joint between strong adherends and mixed mode crack stress fields are rarely absent in typical situations of practical interest.

Consider the joint illustrated in Figure 5(a). A large block of material B is joined by a thin adhesive layer to a block of higher modulus material, A. If A and B are pulled in tension in a direction normal to the joint the x-direction displacements of the crack wall adjacent to A are not in general the same as for the opposite crack wall. Thus the crack surface displacements are not purely in the opening mode. This effect may occur because of a difference in the ratio of ν (Poisson's ratio) to modulus for the two materials, or because of residual stress, or for both reasons. Regardless of the source the additional Mode II crack surface displacements tend to increase the strain energy release available for doing the work of fracture incidental to the separation process.

Theoretical procedures are available by means of which K_I and K_{II} values corresponding to the complex stress field for Figure 5(a) can be calculated. However, for laboratory test specimens and for situations modeling an adhesive joint in service these calculations would be difficult and probably more expensive than a careful experimental analysis designed to furnish the change in compliance with crack extension for the case at hand.

As a means of establishing at least one firm and relatively simple foothold in the adhesive joint testing field it is of value to consider measurement of a critical $\mathcal{G}_I$ value for crack extension in the adhesive joint of Figure 5(b). It is assumed that the adhesive, material B, occupies such a thin layer that y-direction displacements in material A at distances from the crack comparable to the crack length correspond approximately to those which would be found for a crack of similar size and location in a solid specimen of A with the adhesive layer absent. The effect of large strains in B close to the crack edge can then be interpreted as a crack length correction and the presence of the adhesive joint does not add to the complexity of the analysis.

Ripling, Patrick, and co-workers[19] have made measurements on this basis for joints of the aluminum-epoxy-aluminum type. Since it was considered the epoxy would be sensitive to time effects in its response, a crack extension procedure suitable for use at a series of controlled crack extension speeds was needed. Figure 8 shows the specimen configuration which was found to be suitable for this purpose.

The specimen of Figure 6 essentially provides an ability to perform a "peel" test on the adhesive joint while avoiding features such as prior compressive strain or large strains in one adherend which occur in various degrees in "peel" tests as commonly done. At the same time the system is convenient for $\mathcal{G}$ value calculations.

For example in the Ribpling-Patrick work the forces P were exerted through strong fingers separated by a stiff mechanical arrangement virtually equivalent to a wedge. The displacement separation Y of the forces P relative to the zero load position was measured for various crack lengths $\underline{a}$ as a function of the applied load. Thus the compliance C (per unit specimen thickness) could be calculated in accordance with the equation

$$Y = C.P/B \quad \text{- - - - - - - - - - - - -} \quad (7)$$

where B is specimen thickness. From simple beam theory one anticipates

$$C = A\ (a + a_o)^3 \quad \text{- - - - - - - - - - - - -} \quad (8)$$

where $A = 8/EBD^3$

D = depth of beam

a_o = radius of loading hole

E = Young's Modulus of adherent material

The compliance values found were in essential agreement with equation (8) when some allowance was made for an "elastic foundation" contribution to the deflection from the adhesive. The adhesive used was a commercial epoxy often employed as a photo-elastic coating. Comparisons were made with the compliance of a solid aluminum bar with various crack sizes represented by saw cuts. It was decided the results of the experimental stress analysis were best represented by an equation of the form

$$C = A' \, (a + a_o')^m \quad \text{- - - - - - - - - - - - - - - -} \quad (9)$$

where the A', a_o', and m values were adjusted to the data but differed only moderately from the corresponding terms of equation (8). For purposes of this discussion it may be assumed that compliance data in an experiment of this kind can be well enough represented by

$$C = A \, (a + a_o'')^3 \quad \text{- - - - - - - - - - - - - - -} \quad (10)$$

where a_o'' is moderately greater than a_o and represents the effect upon compliance of the strains in the low modulus component, material B.

The crack extension force is given by

$$\mathscr{G}_I = \frac{1}{2} \frac{P^2}{B} \frac{d}{da} (C) \quad \text{- - - - - - - - - - - -} \quad (11)$$

On the basis that the effective crack length during test of an adhesive joint is the value of a consistant both with equation (10) and with the observed values of P and Y, one can compute $\mathscr{G}_I$ for any pair of P,Y observations from the equation

$$\mathscr{G}_I = \frac{3}{2} \frac{PY}{B} \left(\frac{AP}{BY} \right) \quad \text{- - - - - - - - - -} \quad (12)$$

With Y fixed the fractional change of crack-extension force with a is given by

$$\frac{1}{\mathcal{G}}\frac{\partial \mathcal{G}}{\partial a} = - \frac{4}{a + a_o''} \qquad (13)$$

Y fixed

Adhesive joints, as customarily prepared, are non-uniform with regard to such factors as joint thickness, local residual stress, voids, and surface condition. In addition a high polymer adhesive is normally strain rate sensitive in such a way that it tends to respond with greater stiffness and less toughness to rapid straining such as might occur from a rapid increment of crack extension. In attempting to make specimens and measurements of the type discussed here one might find considerable care in joint preparation is necessary in order to produce a specimen which can be partially precracked, say by tapping with a sharp knife blade, without complete separation of the joint. Even a carefully prepared joint will require a certain "stability factor". The preceding term will then be used for the left side of equation (13). For the aluminum-epoxy-aluminum joints studied by Ripling, Patrick, and coworkers, unstable rapid fractures occurred when negative magnitude of the stability factor was appreciably less than -.5 reciprocal inches. Thus stable crack extension occurred from the pre-crack position to a crack length of nearly 8 inches. In order to study substantially weaker, less uniform joints one could employ beams of smaller depth.

Figure 7 shows $\mathcal{G}_I$ values from reference (19) for a series of joint thicknesses with a slow crack speed of about one inch per minute. The portion of the adhesive in greater tension has, of course, some tendency to contract in the specimen thickness direction. To the right of the $\mathcal{G}$ minimum of Figure 8 this tendency was sufficiently pronounced to provide a notching action which kept the crack plane showed a tendency to stay close to one adherent surface and the separation had a rougher appearance. The reasons for the drop of $\mathcal{G}_I$ at very small h values were not specifically determined. Quite likely the cause or causes, if known, would be closely related to the non-uniformities noted above.

Measurements of P and Y were also made with the forces tilted to a 45 degree angle relative to the joint. By using the components of P and Y normal and parallel to the joint it was possible to estimate the division of the total $\mathcal{G}$ value between $\mathcal{G}_I$ and $\mathcal{G}_{II}$. With shearing strains present parallel to the joint not only was the total $\mathcal{G}$ much larger but also the value of $\mathcal{G}_I$ was several fold greater than had been measured under opening mode conditions.

When adhesive joints are used in structural components it is customary to avoid, if possible, loads other than compression and shear on the adhesive joints. Since this is often possible only to an

imperfect degree the critical $\mathcal{G}_I$ value for an adhesive joint is not entirely of academic interest. However, somewhat greater attention is usually given to the strength relative to shear separation. Several values of $\mathcal{G}_{II}$ were measured using specimens of dimensions similar to Figure 8 and with forces applied as indicated in Figure 5(c). For this experiment the compliance is a linear increasing function of crack length and $\mathcal{G}_{II}$ may be computed from

$$\mathcal{G}_{II} = \left(\frac{P}{B}\right)^2 \cdot \frac{1}{ED} \quad \text{- - - - - - - - - - - - - (14)}$$

The resulting $\mathcal{G}_{II}$ values were an order of magnitude greater than the $\mathcal{G}_I$ values shown on Figure 7. Although the instability restraint factor was only half as large as for the Mode I tests a long stable movement of the crack was observed.

It is noteworthy that a crack in a plate of glass is easily extended in a controlled stable fashion by use of splitting forces near the plane of the crack. Although the necessary crack-extension force $\mathcal{G}_I$ is less than 0.1 lbs/in only a small negative magnitude of the stability factor permits control of the crackspeed. It would appear, therefore, that non-uniformities in the adhesive joint rather than low crack toughness per se is the basis for the need of a large negative stability factor in the adhesive joint test specimen.

COMPOSITE STRUCTURES AND MATERIALS

The discovery of occasional fatigue cracks of substantial size is a common occurrence during airplane overhauls. Thus the comet jet airplane failures were not due to crack initiation by fatigue, as claimed at the time, so much as to crack propagation. In present day commercial jet planes this is recognized and the fuselage construction provides for attachment of the frames (or other hoop direction stiffeners) to the pressure shell in such a manner that a rapid tear can be arrested. The simplest types of crack arrestor design employ riveted attachments and the design must permit transfer of forces through the rivets sufficient to hold a section of the pressure shell flat while at the same time reducing the value of $\mathcal{G}$ controlling crack extension to below the value necessary for propagation.

This application of linear elastic fracture mechanics is discussed elsewhere.[20][21] Attention is directed here to the fact that a large field for helpful use of crack stress field analysis methods can be seen in composite structures and materials of current importance.

For example, the basic idea of structures produced by glass filament winding is to embed the glass filaments in a matrix in such a

$$\frac{1}{\mathcal{G}}\frac{\partial \mathcal{G}}{\partial a} = -\frac{4}{a + a_o''} \qquad \text{- - - - - - - - - - - - - - (13)}$$

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For example, the basic idea of structures produced by glass filament winding is to embed the glass filaments in a matrix in such a

way that crack propagation does not occur through the composite on a single plane. Observation of fractures of models shows this is primarily accomplished as follows. As a crack in the bonding resin approaches a glass fiber the adhesive bond to the fiber is weak enough to separate along the approached segment prior to arrival of the crack. Thus the break of each overstressed fiber occurs well away from the local crack plane in the resin. Similar actions occur by debonding of rovings from adjacent rovings and by debonding between layers of rovings.

Although debonding of linear elements seems to be a crude way to introduce toughness into the structure it is surprisingly effective. The detailed study of the debonding influence as well as investigations of plans for improving toughness and strength of filament wound composites in other ways can be substantially assisted by use of the fracture mechanics analysis viewpoints. Although elementary failure mechanism ideas have already been helpful[22] the analytical work desirable for determining optimum balance of properties in filament wound composites lies largely in the future.

EFFECTS OF STRAIN RATE AND TEMPERATURE

After onset of rapid crack-extension, observations of running cracks show the velocity increases with the value of $\mathscr{G}$. However, for $\mathscr{G}$ values in the range of 5 to 10 times $\mathscr{G}_c$ there is relatively little increase of crack speed. The velocity appears to approach a limiting speed of about half the shear wave velocity. Further increase of $\mathscr{G}$ causes branching of the crack in a plate or hackle roughening of the crack embedded in a thick section but no further increase of velocity.[8]

Examination of the effects of material inertia upon the crack border stress field provide little assistance in the understanding of branching of a crack. However, the branching or forking of a crack with increase of K and $\mathscr{G}$ is not unexpected from other considerations. The propagation of a crack in most materials clearly consists in the joining of new origins near the crack border with each other and with the main crack. The elevation of tension at $\theta = 60^\circ$ (Fig. 1) over the tension at $\theta = 0^\circ$ encourages development of new origins away from the plane of the main crack. The roughening of the fracture surface thus introduced is an obvious feature of most fracture surfaces. With extension of a running crack in a large plate, the tensile stress at a fixed distance from the crack border continually increases with K and $\mathscr{G}$. Alternatively, if a critical stress is necessary for development and growth of new fracture initiations in the crack border stress field, the boundary outlining the zone where new origins may occur continually expands in proportion to the value of $\mathscr{G}$. Thus, when the running crack is at or near a constant velocity of extension, both the distance from the crack border of a typical new initiation and the time for its development prior to

arrival of the main crack increase with the value of $\mathcal{G}$. Eventually two new initiations symmetrically placed above and below the crack plane develop sufficiently to stress relieve the region between them thus stopping the main crack and causing the fracture to divide along two branches.

The above explanation of crack division requires that the rate of increase of crack velocity becomes significantly less than the rate of increase of $\mathcal{G}$. On general grounds it seems evident that dc/dt must, eventually, lag behind $d\mathcal{G}/dt$ and may, in fact, approach a limiting constant value. For example it was suggested by Dr. E. A. Saibel[23] that the limitation on $c < 0.5\ c_s$ may stem from the fact that the acoustic signal velocity in a liquid is commonly about half its value in the solid form of the material. Thus, in the zone of large non-linear strains and new crack origins near the crack border, the material may be sufficiently disordered so that stress waves are not propagated through this zone more rapidly than $0.5\ c_s$. In this event the crack speed may reach that limiting velocity prior to the beginning of crack division. This, in fact, is observed.

In summary it appears that crack division occurs because new origins ahead of the crack and away from the crack plane are natural to crack propagation and because the increase of crack velocity with increase of $\mathcal{G}$ is limited by the stress-wave signal-velocity limitations of the growing zone of disordered material near the border of the main crack. The velocity limitation thus imposed upon crack speed is so far below the stress wave velocities for the material away from the crack that the region of elastic strains surrounding the fracture process zone can be regarded as essentially the same stress pattern as that which is observed for the stationary crack.

Consider next a plastic zone at the leading edge of a running crack as suggested in Figure 1. Assume the equilibrium conditions for validity of the constant flow model are maintained. Thus X is constant and the patterns of strain and temperature are time invarient. Assume also the plastic strains are governed by flow laws of plasticity theory with no allowance for temperature and strain rate effects other than through their influence upon the yield stress σ_{Ys}. The linear dimension of the plastic zone can be taken as proportional to $(K/\sigma_{Ys})^2$.

Now, if one were to compare the strain patterns for various values of crack speed c and temperature T one would see nothing distinguishing one pattern from another except the plastic zone dimensions. Clearly the work rate $\mathcal{G}$ will be proportional to zone size times $(\sigma_{Ys})^2$. Examination of this product provides only the information, already known, that $\mathcal{G}$ is proportional to K^2. The constant flow model does not at this point permit one to predict the trends of $\mathcal{G}$ with variations of

σ_{YS}, T, and c which are of interest. For example, the experimental data suggest that, with c constant, an increase of σ_{YS} through decrease of temperature results in a reduction of $\mathscr{G}$. Clearly additional factors and relationships linking the flow and fracture mechanisms in the plastic zone to c and to T are needed.

From a dynamic viewpoint, with Saibel's suggestion in mind, a relationship might be found through study of the velocity for transfer of a mechanical disturbance in the plastic zone. From a static viewpoint, a relationship might be introduced by considering the influences of temperature and strain rate upon strain hardening. At present the analytical methods which will eventually clarify this feature are not in good focus. Experimental techniques for collecting data which would assist this task are reasonably well developed and the examination of substantial amounts of experimental information will be needed as a basis for future analytical work.

Figure 8 shows a limited amount of data for the onset of fast fracture toughness K_{Ic} with temperature indicated by this data may be typical for alloy steels of the high yield strength type.

Although it may seem unlikely that the K_{Ic}-T relation is always smooth such data as has come to the writer's attention shows no indication of a jump in K_{Ic} values over a narrow range of temperature. Krafft and Sullivan[24] have noted that the tendency of low carbon steel toward twinning at low temperatures has a definite influence upon the K_{Ic}-T curve. Their paper gives the results of K_{Ic} measurements on several low carbon steels at a series of loading speeds and temperatures. They suggest a very simple relationship of the form

$$K_{Ic} = C\,\bar{\sigma}_Y^{\,3/2} \qquad \text{- - - - - - - - - - - - - -(15)}$$

may represent both the temperature and strain rate influence with useful accuracy. To show agreement of this relation with data σ_Y must be interpreted as the upper yield stress for the prevailing test temperature and for the stress rate formally calculated from the elastic theory stress pattern at a small fixed distance from the crack border. Equation (15) is of value as an empirical rule for interpolating or extrapolation measurement data. It is not possible at this writing to suggest a theoretical model leading in a clear way to equation (15). A large amount of additional data in this general area is needed for both practical reasons and to guide the construction of appropriate theory.

COMMENTS ON THE RATE THEORY APPROACH TO FRACTURE

It has long been known that the tensile strengths of certain pure liquids can be estimated roughly just from molecular size and surface tension. These estimates have been refined by treatments of nucleation and rate theory aspects of liquid tensile strength. Clearly our theoretical understanding of strength of pure liquids exceeds that pertaining to strength of solids in any degree of purity. Since the writer cannot add appreciably to an account of the topic published in 1958[8] the remarks given here can be quite brief.

It is of interest to recall the following points. (1) The activation energy for a cavity of critical size in the pure liquid is not stress independent but varies inversely with the square of the applied tension. (2) Time at load and test volume seem to have a complementary relationship in their effects upon the predicted strength level. (3) The influence of flaws at the boundary or impurities within the liquid, even in small amounts, lowers the tensile strength far below the strength value which can be obtained when these are essentially eliminated. All of these features seem to possess counterparts in the flow and fracture of solids. Further study of the liquid tensile strength problem might add other features of interest.

Along the road toward a rate theory basis for fracture strength of real solids one can start in a sound way by providing observations of energy dissipation rate with crack extension as function of time and temperature. Focusing attention on the zone of large non-linear strains near the crack border one can then contemplate the modeling and separate study of the non-linear strains in ways which would permit proper allowance for influences of straining speed, dimensions, and strain programming. Beyond this point lies a struggle with the flaw population. The degree to which flaws of other kinds will mask or disturb the influence of dislocations and vacancies must somehow be determined. Finally, during investigation of the problem on the size scale of dislocations, treatments of the small separation events in ways suggested by the liquid tensile strength problem should not be overlooked.

TABLE I

Analytical Tools		Observations	
A1	Flaw-Statistics	B1	Critical Fracture Stress
A2	Linear-Elastic Fracture Mechanics	B2	Crack Propagation Toughness
A3	Plasticity Mechanics	B3	Fracture Examinations
A4	Dislocation Mechanics	B4	Separation of Adhesive Joints
A5	Nucleation Analysis of Micro-Cracks and Cavities	B5	Crack Growth Rate vs K (Stress plus Environment) (Fatigue)
A6	Theory of Rate Processes	B6	Observations of Micro-Cracks and Cavities
		B7	Observations of Dislocation Behavior

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TABLE II

Utilitarian Applications

C1 Fracture Failure Analysis and Reliability

C2 Development of Crack Toughness Standards

C3 Crack Toughness Aspects of Materials Development

C4 Control of Crack Extension Due to Stress-plus-Environment and to Fatigue

C5 Control of Particle Size and Energy Loss in Comminution

C6 Estimates of Cracking in Rocks and Soils

C7 Control of Creep and Stress Rupture Failures

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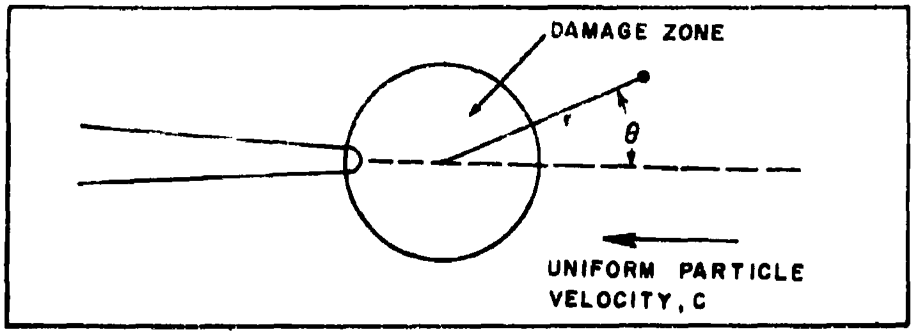

FIGURE 1. STEADY FLOW MODEL OF A CRACK BORDER DAMAGE ZONE

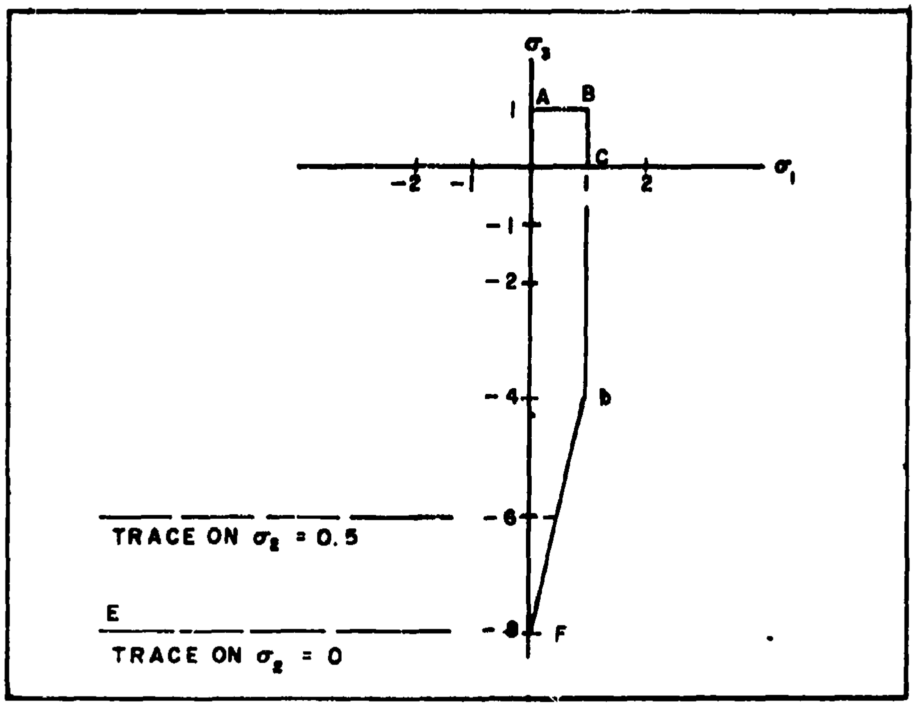

FIGURE 2. CRITICAL FAILURE STRESS LOCUS LINE ABCD IS SHOWN ASSUMING K_{IIc} IS TWICE K_{Ic} AND THE FRICTION COEFFICIENT f IS 0.75

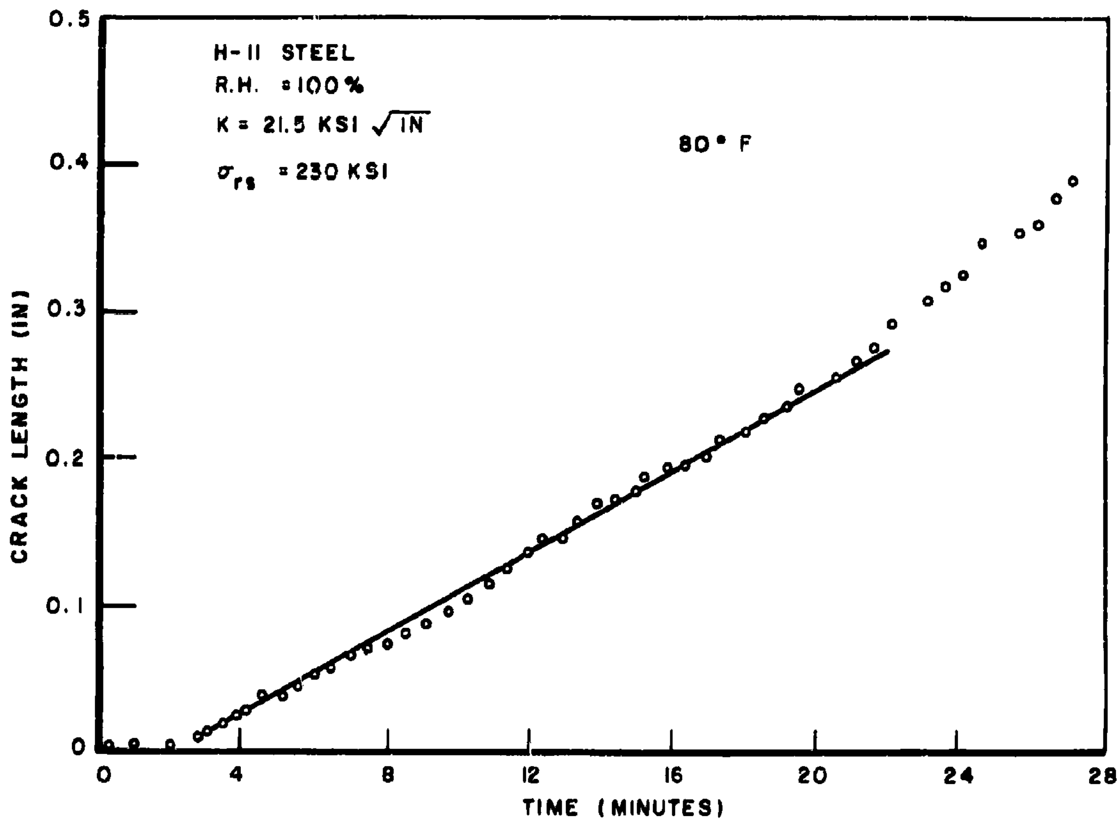

Figure 3. Crack extension in a high strength steel as a function of time with the K value held constant and distilled water in contack with the material at the crack border. (Preliminary data from H.H.Johnson, Cornell University).

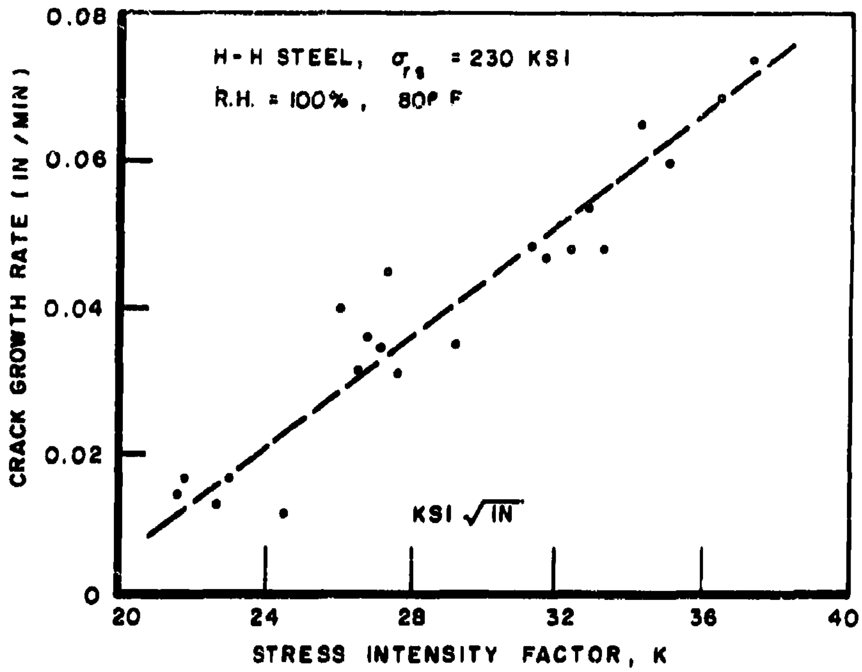

Figure 4. Crack growth rate measurements in a high strength steel as a function of K value. Distilled water was in contact with the material at the crack border. (Preliminary data from H.H.Johnson - Cornell University).

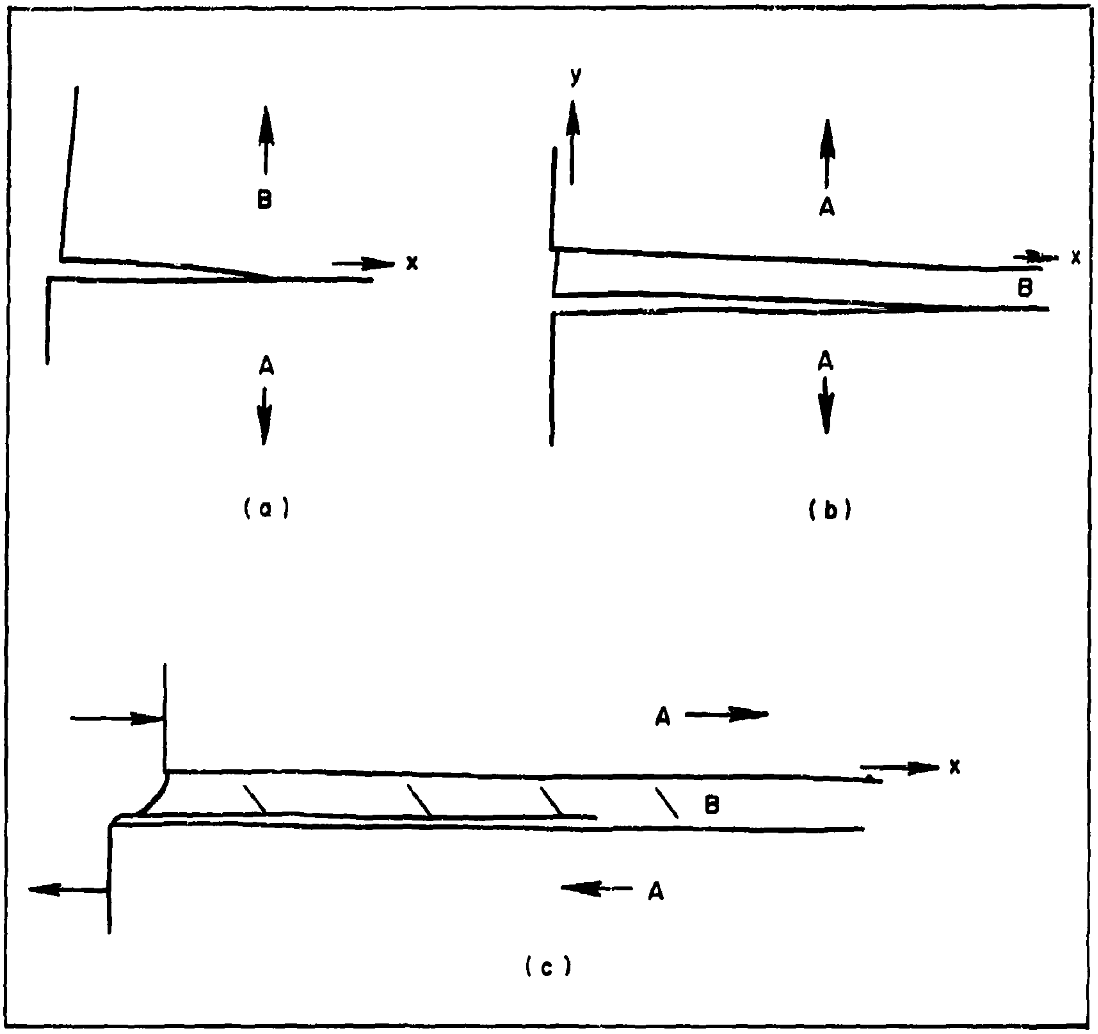

Figure 5. Separations from the left boundary in adhesive joints: (a) a thick block of material B joined to a more rigid material A; (b) and (c) a thin layer of material B between block of material A.

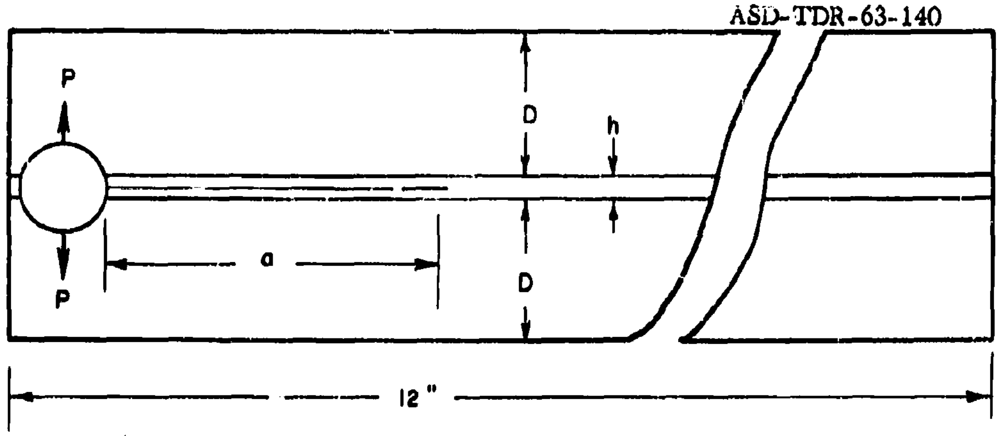

Figure 6. Ripling-Patrick specimen for $_{I}$ and $_{II}$ measurements on adhesive joints. Most of the specimen bars were 12" long and 1/4" thick but these as well as other dimensions are arbitrary choices.

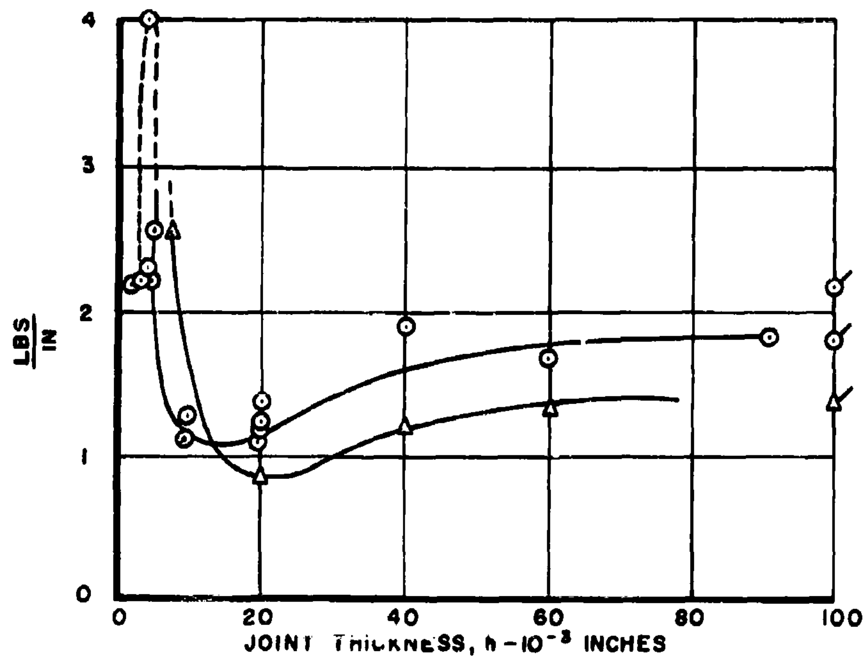

Figure 7. Effect of joint thickness H upon critical $_{I}$ values for progressive separation. Circle points were measured with the crack speed roughly one inch per minute. Triangle points were measured with the speed increased by a factor of 25. Points on right margin were measured with solid epoxy specimens.

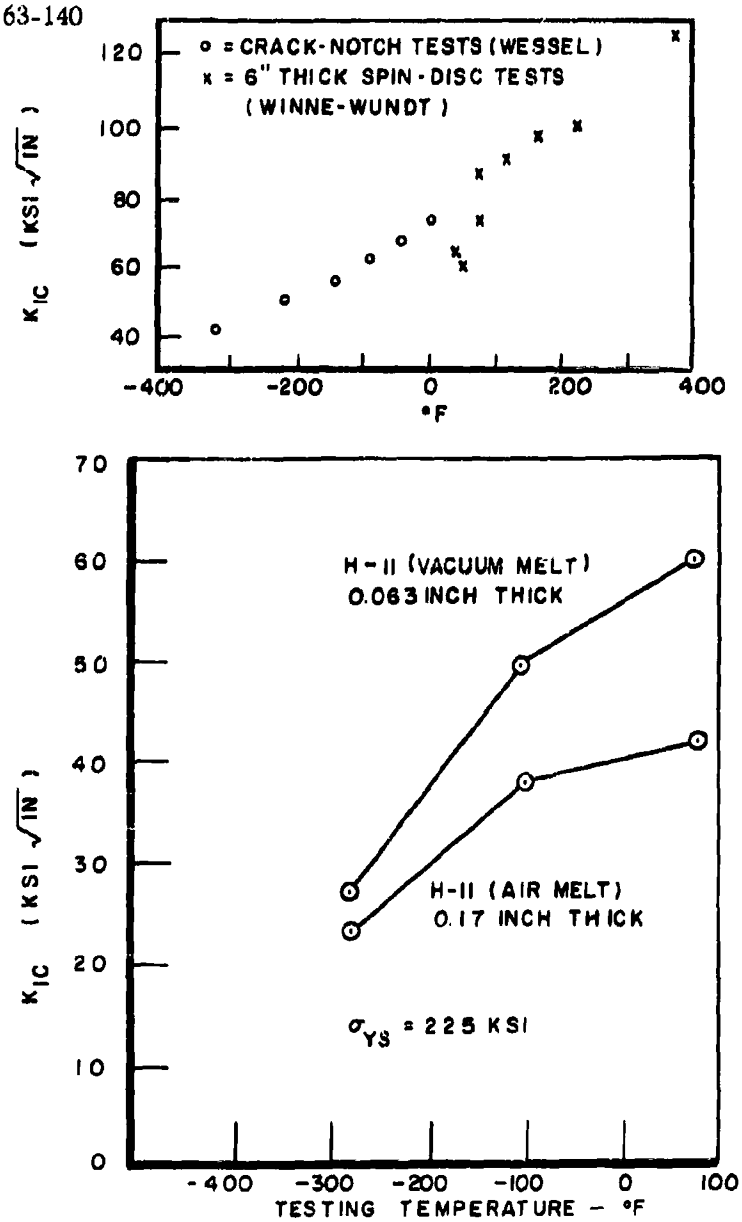

Figure 8. K_{Ic} as a function of temperature for several steels. Upper graph shows data replotted from Winne and Wundt (squares) taking specimen thickness into account. On same graph is data from Wessel obtained with edge notched tensile tests (circles). Shown below are K_{Ic} values from Sprawley and Smith obtained using part-through cracks.

DISCUSSION

DR. PLASS

I am sorry to say we will not have time for a discussion period following this talk. We are running a little bit overtime in our session. I would like to proceed immediately to the second paper which is titled "Time Dependent Fracture," and will be given by Dr. C. C. Hsiao of the University of Minnesota. He received his early training in China and his graduate work was done in this country at MIT and the University of Colorado. He is a member of the American Physical Society, the Society of Geology, and several others. We will proceed immediately to Dr. Hsiao.

DR. HSIAO

Mr. Chairman, lady and gentlemen. When I was asked to give a paper at this symposium, I thought that this material might be of interest to this audience. At least I feel, personally, that the time dependent of fracture is of fundamental importance as well as a practical one. In the remote past the analysis of the basic strength of a solid body was based upon the conception of a crystal character of the break.

TIME DEPENDENT FRACTURE

by

C. C. Hsiao, Ph.D.

University of Minnesota

(This paper supported in part by the U. S. Atomic Energy Commission.)

TIME DEPENDENT FRACTURE

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ABSTRACT

A brief account is given of some recent efforts here and abroad in studying the nature of rupture of solids. One of the important factors has been found to be the variation of time in the process of destruction of the solids. For a large variety of solids both in experiment and in theory the logarithm of time has shown to be linearly related with the applied uniaxial state of tensile breaking stress. This linear law has been established at least for some solids over a time range of a dozen decades from microseconds through months. Some suggestions are also given for future investigations on the time dependent fracture of solids.

TIME DEPENDENT FRACTURE

INTRODUCTION

In the remote past, the analysis of breaking strength of solid bodies was based upon the conception of a critical character of the break. It was believed that a body would break instantly under a critical threshold tensile force. Below that threshold value the body would last for a long time if not indefinitely without failure. However, many relatively new experimental facts have indicated that the rupture of solids is by no means a simple phenomenon even for the case of brittle fracture under uniaxial tension. The magnitude of breaking tension is found to be intimately tied in with the duration of load application or the rate of loading. As a general rule, the shorter a solid body is in a stressed state, the larger the load is necessary to break it. This clearly shows that we cannot assume that a solid body will fail under a critical threshold tension, but that the nature of breaking is associated with some gradual developing processes in a stressed body which take time. Thus, apart from many other factors such as temperature and composition of medium that affect strength, it has been well established that time is an important parameter in the study of the strength and fracture of solids. As a result during the past fifteen years many scientists have centered their attentions in investigating the time dependent fracture phenomenon of solids.[1-9] The following is a brief account of some of the experimental and theoretical progress and achievement in the understanding of the subject matter. Some suggestions are also given for possible future research on the time dependent fracture of solids.

SECTION I: EXPERIMENTAL RESULTS OF TIME DEPENDENT TENSILE STRENGTH

Up to now the time required for rupturing a solid under stress can only be determined from actual experience. There are no data available that one can estimate the time taken for a solid to fail under any given state of stress. However, one exception is for the simplest state of uniaxial tension for which the strength dependency of time is becoming relatively clear, at least for some solids. Based solely upon many experimental facts the time t taken for a solid to failure under a constant applied simple tensile stress σ has been found by Zhurkov and his co-workers to obey a general law that[3]:

$$t = t_o e^{\frac{u_o - \gamma\sigma}{kT}} \tag{1}$$

where t_o, u_o, γ and k are constants and T is the absolute temperature. This relatively simple relationship between time, temperature and applied tensile stress has been determined for a large variety of solids with different physical configurations and chemical compositions. Because of the interesting nature of this investigation the writer thought of reviewing some of Zhurkov's work and pointing out some of the important aspects of his results as well as some short-comings. Fig. 1 shows the dependence of the breaking tension on time for metals, alloys, silver chloride, polymers etc. The straight lines indicate that for a constant temperature there exists a linear law between the logarithm of time and the applied tensile breaking stress. i.e.

$$\log t = A - B\sigma \tag{2}$$

where A and B are constants dependent upon temperature and composition of the substance. With T constant both equations (1) and (2) represent the same relationship between the time to failure t and the applied tensile stress σ.

According to Zhurkov and his colleagues[10,11,3,4], Eq. (1) does not represent a common simple empirical correlation but reflects a deep physical process of destruction which occurs in a stressed solid body. The coefficient t_o was found to be essentially constant for most metals whose magnitudes coincided with the period of natural oscillations of atoms in the metal lattice. An activation mechanism of the process of destruction was considered. The duration of this process was determined by the temperature T and the magnitude of the activation barrier $u = u_o - \gamma\sigma$. The smaller was the applied stress to a solid, the greater was the activation barrier to overcome in order that the solid could reach its state of rupture. What happened to be especially notable was that the energy which has been determined on the basis of the time and temperature dependent strength of pure metals such as aluminum, nickel, platinum, silver and zinc, has coincided with the sublimation energy of bonds of atoms in a crystal lattice.

Fig. 2 shows that the quantity t_o may be determined by the intersection of all the straight lines for different temperatures through extrapolation. At the intersection the time to failure becomes independent of temperature which indicates that $u_o - \gamma\sigma = 0$ and $t = t_o$. Using aluminum and aluminum alloy (with 2% Mg) as shown in Fig. 3 and Fig. 4 that the quantity $t_o = 10^{-12}$ sec. is independent of the microstructure and the chemical composition and is close to the period of natural vibrations of atoms in the solid. However, from Zhurkov's data[10], t_o was found to be 10^{-9} sec.

for platinum. It is not known whether this represents the actual period of natural oscillations of the platinum atoms in their lattice.

Now with t_o as a constant we can obtain from Eq. (1) that:

$$\log \frac{t}{t_o} = \frac{u}{KT} \qquad (3)$$

where $u = u_o - \gamma\sigma$. As suggested by Zhurkov, by plotting log t with respect to the function of 1/T one can get a family of straight lines whose slopes represent the variations of the quantity u, the activation energy. As shown in Fig. 5 if we extrapolate the lines between applied stress σ and activation energy u for zero stress, then u_o can be obtained. It is interesting to note that this quantity u_o is not affected by either heat treatment or composition of the solid. As a result, since $u = u_o - \gamma\sigma$, the quantity γ represented by the slopes of the straight lines is affected by the structural changes and composition and thus the increase of strength as shown in Fig. 3 and Fig. 4. The parameter γ seems to be tentatively the only quantity which governs the dependence of strength on the time to failure at any given temperature. This is clearly brought out by Figs. 6, 7 and 8 for aluminum, zinc and Al + 2% Mg respectively at various annealing temperatures. For the same times to fracture the higher is the value of γ of a solid the lower is its mechanical strength. However, in checking Fig. 3 and Fig. 6 for annealed aluminum the data reported by Zhurkov are apparently the same, but the straight lines in Fig. 3 obtained at constant test temperatures do not seem to converge at zero stress level as indicated in Fig. 6. Also as shown in Fig. 3

all the straight lines intersect at a time scale of 10^{-12} sec., but the same data plotted in Fig. 6 do not seem to follow exactly the same pattern. It is not known whether such discrepancies are resulted in the extrapolation or they simply are inherited in the natural behavior of the solids.

Without considering the discrepancies between the relations represented by Eq. (1) and many reported data that are not following this general time-dependent behavior, still there is an uncertainty as to the meaning of the finite time to fracture at zero applied stress. Bartenev has proposed a theory[2] to remove this undesirable point. But the exact nature of this complicated fracture phenomenon remains to be explored. Nevertheless it appears highly important to continue somewhat similar investigations to clear up some of the fundamental understandings of this time dependent fracture phenomenon.

In order to illustrate the structural dependence of the quantity γ, Fig. 9 shows the variation of γ on the grain size[4]. This was also reported in Zhurkov's work and it was found that the quantity γ was linearly proportional to the square root of the grain size for several metals investigated. For the same times to fracture the smaller was the grain size, the less was the value of γ and thus the greater the mechanical strength. This was claimed to be reasonable from the standpoint of the accumulation of dislocations and local stress concentrations at the grain boundaries. Such local stress concentrations would develop local discontinuities and cracks which in turn would grow and eventually led into a complete failure of the solid body.

Similarly there has been another simple relationship

observed between the applied tensile or compressive stress for a solid and the rate of straining. For any given strain the stress obtainable at a fixed temperature for different rates of straining is linearly related with the logarithm of the strain-rate.[5,12,13,14] Mathematically, if ϵ is the given strain at any given temperature T, then the stress σ is related with the strain-rate $\dot{\epsilon}$ in the following manner:

$$\dot{\epsilon} = \dot{\epsilon}_o e^{\frac{\sigma-\sigma_o}{m}} \Big|_{\epsilon} \qquad (4)$$

where $\dot{\epsilon}_o$ and σ_o are associated constants and m is the slope of the straight line when log of strain rate is plotted against the stress. This relationship is found to be quite useful in studying the time-dependent strength properties of solids as it is relatively easier to carry out tests with either constant or varying strain-rates. It is also useful for obtaining the quantity time associated with the strength properties of a solid. At any constant strain-rate $\dot{\epsilon}$, Eq. (4) can be put into the following form:

$$t = \frac{\epsilon}{\dot{\epsilon}_o} e^{\frac{\sigma_o-\sigma}{m}} \Big|_{\dot{\epsilon}} \qquad (5)$$

This is the time-stress relationship at a constant strain rate $\dot{\epsilon}$. Here ϵ can be used as a measure of molecular configuration of the solid.[15] The time-dependent stress σ is also related to the quantity ϵ which is the strain of the solid in simple tension. As a special case when ϵ is the fracture strain, then t will be the time to fracture and σ

the stress required at fracture. This agrees fairly well with the result obtained by Zhurkov.

In order to check the linear law between log of time and fracture stress over a time range of a dozen decades, data obtained from different sources under different test conditions were converted and plotted. Fig. 10 shows the results thus obtained on the time-dependent fracture strength for soda glass, polymethyl methacrylate, and polystyrene solids. It is interesting to observe that for all the three materials investigated the linear law between the logarithm of time to failure and the applied tensile breaking stress seems to hold. This covers the time range from the microsecond region to nearly a year. In general, the convenient time range obtainable from constant strain-rate tests is between a few minutes to a few hours. In order to extend the time range, creep tests under static loading conditions can be used for long time fracture information from hours to months. For very short time fracture strength only limited data have been obtained with application of the pulse technique using explosives[16]. The data used in Fig. 10 in the microsecond region are from Kolsky's work.[9]

In addition somewhat similar attempts have also been made in the USSR and indirectly reported by Bartenev and others[8,17] in confirming the linear relationship between the logarithm of time and the applied tensile breaking stress. Fig. 11 shows the results for a metallic solid as well as for a polymer solid obtained from direct measurement of time to fracture under a constant applied stress, from data of continuous stressing and from data of cyclic stressing.

SECTION II: THEORETICAL STUDIES OF TIME DEPENDENT TENSILE STRENGTH

While the experimental investigations on the time and temperature dependent tensile strength were being pursued, theoretical studies of the same subject have also been followed.[1,2,18,19,20,21] Most of the theoretical analyses on time dependent strength were based upon phenomenological microscopic considerations. An attempt was also made to tie the molecular configuration into the variation of the mechanical strength.[15]

As earlier as 1947, Saibel[1], in a short note, had proposed to treat the problem of fracture of solids from the point of view of chemical reaction rate considerations. For a steady load application, assuming that the force tending to break the bond between ions or atoms increases with the number of bonds which have already been broken, the relation between the initially applied stress σ and the time to fracture t was found to be:

$$\sigma = C \log \coth Dt \tag{6}$$

where C and D are constants dependent upon temperature. For long times Eq. (6) may be written:

$$\sigma = 2Ce^{-Dt} \tag{7}$$

For short times, the effect of rate of loading is predominant, and σ may be expressed in the form $\sigma = \dot{\sigma}t$ for a constant rate of loading. Assuming the rate of loading is high then to a first approximation

$$t = \frac{\log E\dot{\sigma}}{F\dot{\sigma}} \tag{8}$$

where E and F are constants depending on temperature.

Similarly Bartenev[2] also used the conception of the dependence of the potential energy across the interatomic distances in the process of breaking and developed a theoretical consideration of the time dependent strength of solids. Using again Eyring's absolute reaction rate theory and the modified frequency of fluctuations which lead to the breaking and reforming of the cracks, the dependence of time for fracturing on stress and temperature was also formulated. The result coincided with the empirical relation as shown in Eq. (1). One feature is interesting in that the analysis takes the size effect of the sample into consideration.

On somewhat similar basis, Bueche[18,19] had considered the tensile strength of polymer solids. A simple model sample was constructed with a large number of molecular bonds point only in three mutually perpendicular directions. If the tensile force σ per unit area is applied to the sample in one of the three directions with n_o bonds per unit area pointing in that direction, then it can be shown that the number of bonds per unit area unbroken after a time t is given by

$$n = n_o e^{-p\omega t} \tag{9}$$

where $p = e^{-\frac{E-f\delta}{kT}}$. The quantity f is the tension in the bond, E, the bond energy, ω, a segment oscillation frequency and 2δ is approximately the distance the bond will stretch

before breaking. From Eq. (9) the tension on each unbroken bond is

$$f = f_o e^{p\omega t} \tag{10}$$

where $f_o = \frac{\sigma}{n}$ is the tension per bond. To a good approximation assuming that $p\omega t = 1$ when the solid fails, then the time dependent tensile strength will be determined by the following relation

$$t = \frac{1}{\omega} e^{(E - 2.72\frac{\delta}{n}\sigma)/kT} \tag{11}$$

where t is the time taken for the sample to break under the given applied stress σ. This is again similar to the experimental relationship shown in Eq. (1).

Coleman[20] also made an intensive theoretical analysis of the time dependent mechanical breakdown phenomena. The phenomenological theory presented is applicable to creep failure of oriented polymer filaments under tensile stresses. In using the theory one makes assumptions about the distribution of breaking times in ensembles of filaments which are bearing constant loads and then proceeds to calculate the distribution of time to failure under other stress histories. The analysis permits a calculation of the dependence of observed tensile strengths on both the sample size and the rate of loading.

As an approximation by neglecting some statistical considerations he also formulated a simplified absolute reaction theory.[21] The time to failure of a fiber which is subjected to a constant tensile stress σ is given by

$$t = \frac{\gamma_B}{\lambda} \frac{h}{kT} e^{\Delta F/RT} e^{-\delta\sigma/2kT} \tag{12}$$

where ΔF is the free energy of activation for the displacement of force centers, λ is the separation between the positions of minimum potential of mean force, δ is equal to the product of the effective cross-sectional area per force center and the jump distance, γ_B is a measure of the critical amount of irreversible distorsion that the microstructure can tolerate before the breakdown process occurs with catastrophic rapidity, T is the absolute temperature, R is the gas constant, k is Boltzmann's constant and h is Planck's constant. Eq. (12) can easily be converted to the following form

$$\log t = A - B\sigma$$

which is the same as the empirical Eq. (2). Here A and B are functions only of T.

Another version of the analysis of the time dependent strength was also put forward by Pines.[7] The time dependent fracture phenomenon was explained as diffusional growth of existing small nuclei of cracks up to a critical size, after which the cracks started to grow at an accelerated rate, leading to the destruction of the solid body. For the calculation of the kinetics of the growth of the cracks the local change of elastic energy during the displacement of each vacancy was taken into account as it determines the change of the activation energy in the process. The differences in the rates of flow of vacancies from the body to the crack and from the crack to the body were also considered. The final approximation resulted in an expression of time as functions of temperature and applied stress. However, it does not seem to change very much the basic pattern of the time dependent nature of the fracture strength.

SECTION III: SOME REMARKS AND SUGGESTIONS FOR FUTURE RESEARCH

From both the experimental data and the theoretical analyses it seems reasonably clear that we do have some understanding of the one dimensional time dependent tensile strength for solids. However, for any other state of stressed solids we have neither any basic understanding nor any workable empirical relationship. It appears that we must sooner or later break through this simple state of tensile strength barrier and obtain information concerning a solid with more than one dimensional state of strength. This problem is a difficult one, but certain limited progress toward this end may be easily made for several relatively simple states of stressing such as shear or biaxial tension. After a fundamental consideration of the time dependency on a combined state of stressed solid becomes clear, even a solid body under triaxial tension can be handled theoretically.

Much of the work up to date has been directed toward the study of the brittle fracture behavior of a tensile specimen. The ductile behavior of which is very dependent upon the orientation of the specimen. For both polymers and metals the anisotropic microscopic configurations of the molecular bonds will no doubt initiate dislocations and thus crack formations not the same as those in an isotropic solid. The study of the molecular orientation on the time dependent mechanical strength will be of great interest. Some work[15,22] has already been done on the orientation effect upon strength of polymers. Although the application of the theory to metal structures is possible, however, the introduction of the time dependency into the picture of fracture strength remains to

be worked out.

In examining the stress and strain-rate expression as given in Eq. (4)

$$\dot{\epsilon} = \dot{\epsilon}_o e^{\frac{\sigma-\sigma_o}{m}}\Big|_{\epsilon} \quad (4)$$

an interesting relationship can be deduced for the product of the time to fracture of a solid and its creep rate. The time to fracture expression can be obtained as a special case of Eq. (4), when ϵ is the fracture strain and $\dot{\epsilon}$ = constant, then

$$t = \frac{\epsilon}{\dot{\epsilon}_o} e^{\frac{\sigma_o-\sigma}{m}}\Big|_{\dot{\epsilon}} \quad (5)$$

where t is time to fracture, ϵ is the fracture strain, σ, the fracture stress and $\dot{\epsilon}_o$ and σ_o are constants.

For constant rate creep under any constant stress σ, the creep rate and stress relationship can be expressed as follows:

$$\dot{\epsilon}_c = \dot{\epsilon}_o e^{\frac{\sigma-\sigma_o}{m_c}} \quad (13)$$

where $\dot{\epsilon}_c$ is the creep rate at a constant applied stress σ, and $\dot{\epsilon}_o$ and σ_o are the constants used in Eq. (4) and Eq. (5), and m_c is the slope of the straight line plotted with creep rates against creep stresses. Therefore the product

$$t\dot{\epsilon}_c = \epsilon e^{(\frac{1}{m} - \frac{1}{m_c})(\sigma_o-\sigma)} \quad (14)$$

If $m = m_c$ as this is true for a number of metals and alloys then the product $t\dot{\epsilon}_c = \epsilon$ which is just the fracture strain, a constant independent of temperature.

However, if m is not equal to m_c as pointed out in an earlier report[5] and also reported by many other investigators[23], the significance of the product of time to fracture and the creep rate of a solid becomes more difficult to interpret. But with the help of the results reported by Zhurkov and Sanfirova[24], we may rewrite respectively Eq. (5) and Eq. (13) as

$$t = \frac{\epsilon}{\dot{\epsilon}_o} e^{\gamma \frac{\sigma_o - \sigma}{kT}} \tag{15}$$

and

$$\dot{\epsilon}_c = \dot{\epsilon}_o e^{\gamma_c \frac{\sigma - \sigma_o}{kT}} \tag{16}$$

where γ and γ_c are material constants depending upon the cold work, annealing, orientation, any structural variation etc., as well as the composition of the solid. Then the product is simply:

$$t\dot{\epsilon}_c = \epsilon e^{(\gamma - \gamma_c)\frac{\sigma_o - \sigma}{kT}} \tag{17}$$

indicating that it is dependent upon the variation of the material constants and thus the temperature, assuming the composition of the solid is invariant. This means that under different conditions of testing we may expect the variation of the material constant. This is reasonable because the material changes constantly during testing. If this

variation of the material constant can be determined, then a definite correlation between the time to fracture and the creep rate can be established. This is of great importance to us as for both the very long time as well as the very short time behavior of a solid when the time to fracture is not directly practical for determination, one can estimate them with the help of the relatively short duration creep data.

Speaking of very short time behavior of a solid, to the writer's knowledge, the shortest time to fracture obtainable has seemed to be in the neighborhood of the microsecond region. Referring to Fig. 10 again, the time dependent strength has been obtained from very high rates of loading using lead azide for the initiation of short pulses. This has been done only for the most simple state of stress and only for a few sample materials. It would seem desirable to extend the scope of this type of experiments to cover a large selection of solids under different states of stressed conditions. Currently some oriented and unoriented materials are being investigated in the writer's laboratory using the pulse technique. It is hoped that the results thus obtained will shed some more light on the effects of anisotropy and state of stress upon the time dependent fracture phenomenon.

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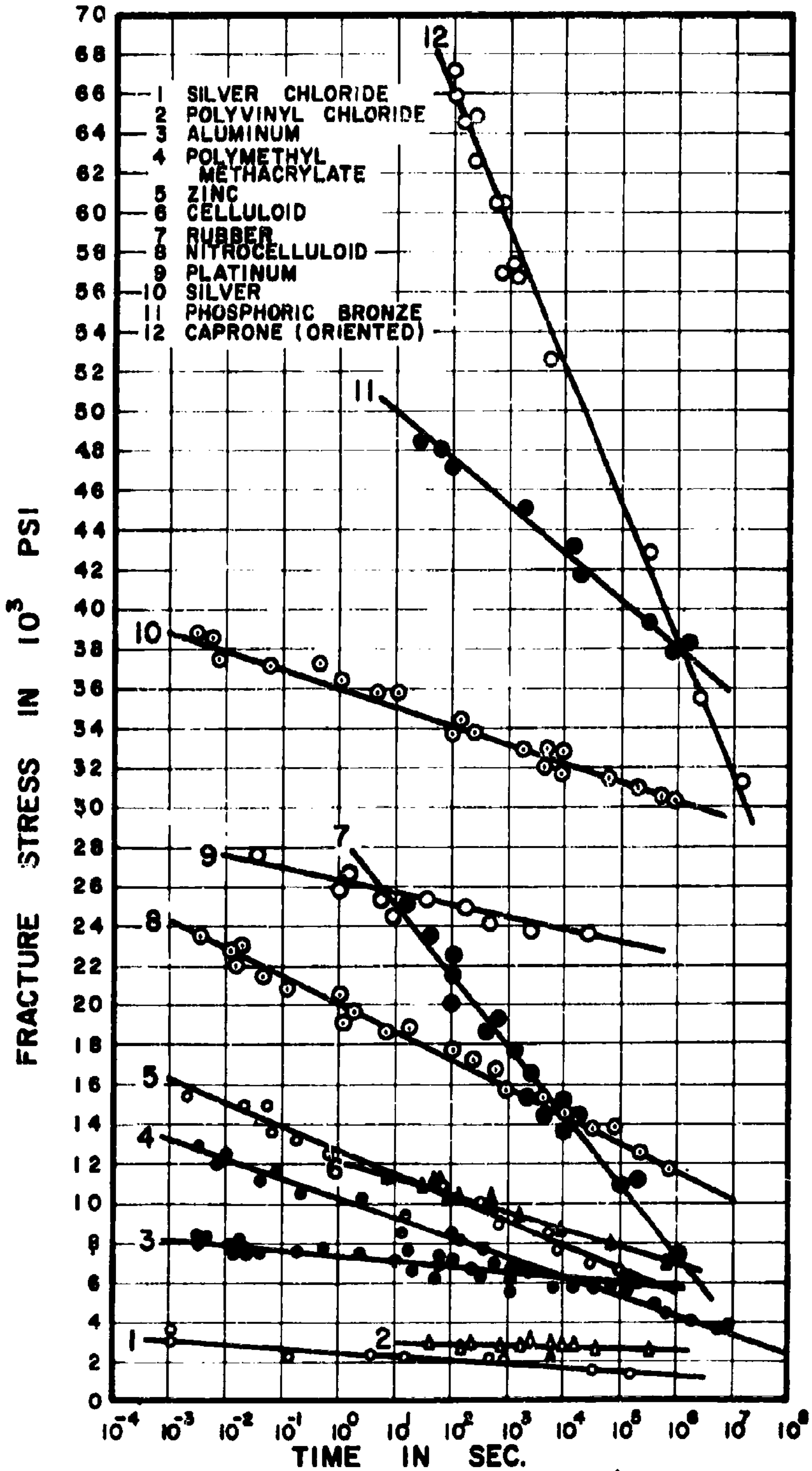

Fig. 1 Time Dependent Strength of Solids (after Zhurkov)

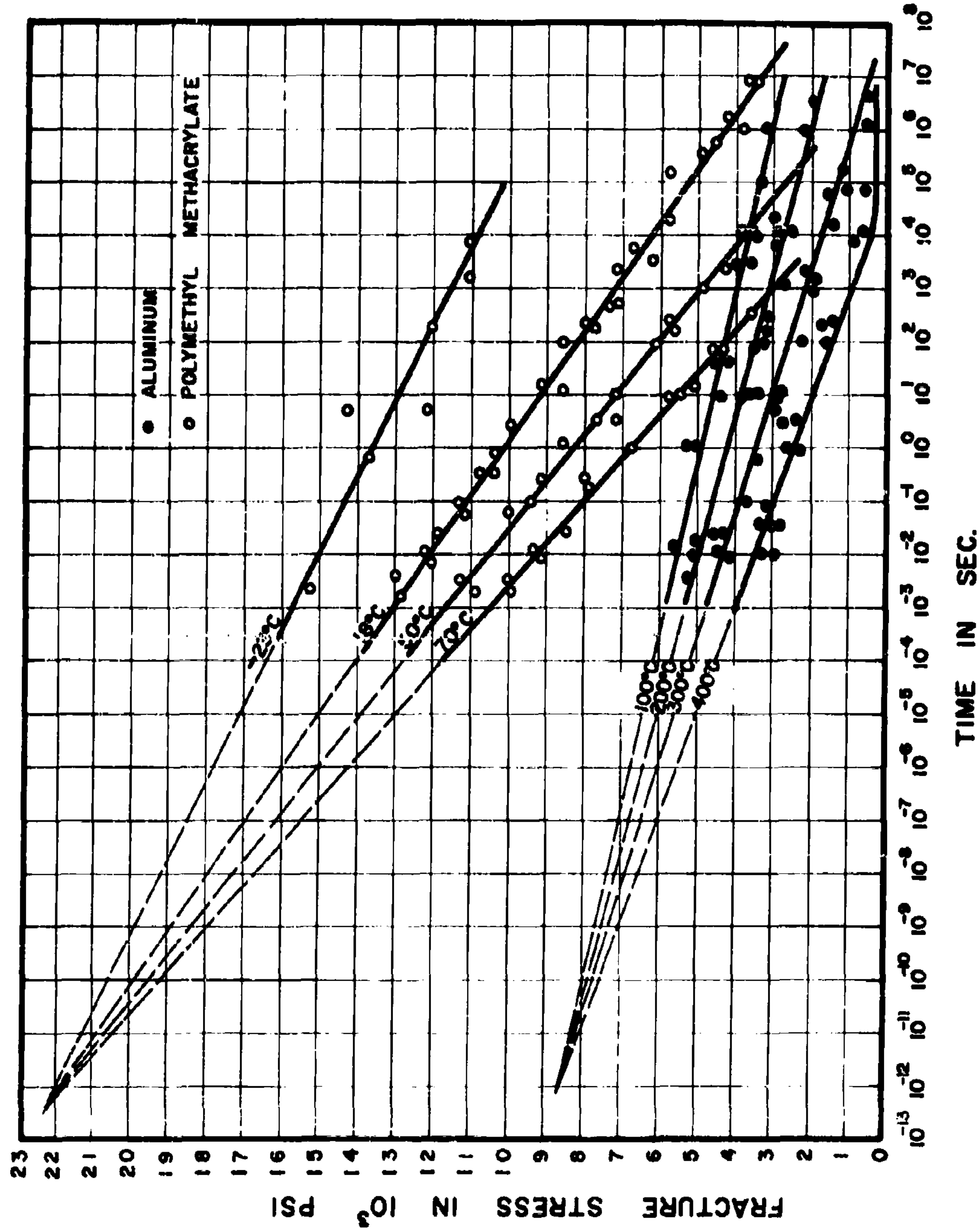

Fig. 2 Temperature and Time Dependent Fracture of Solids (after Zhurkov)

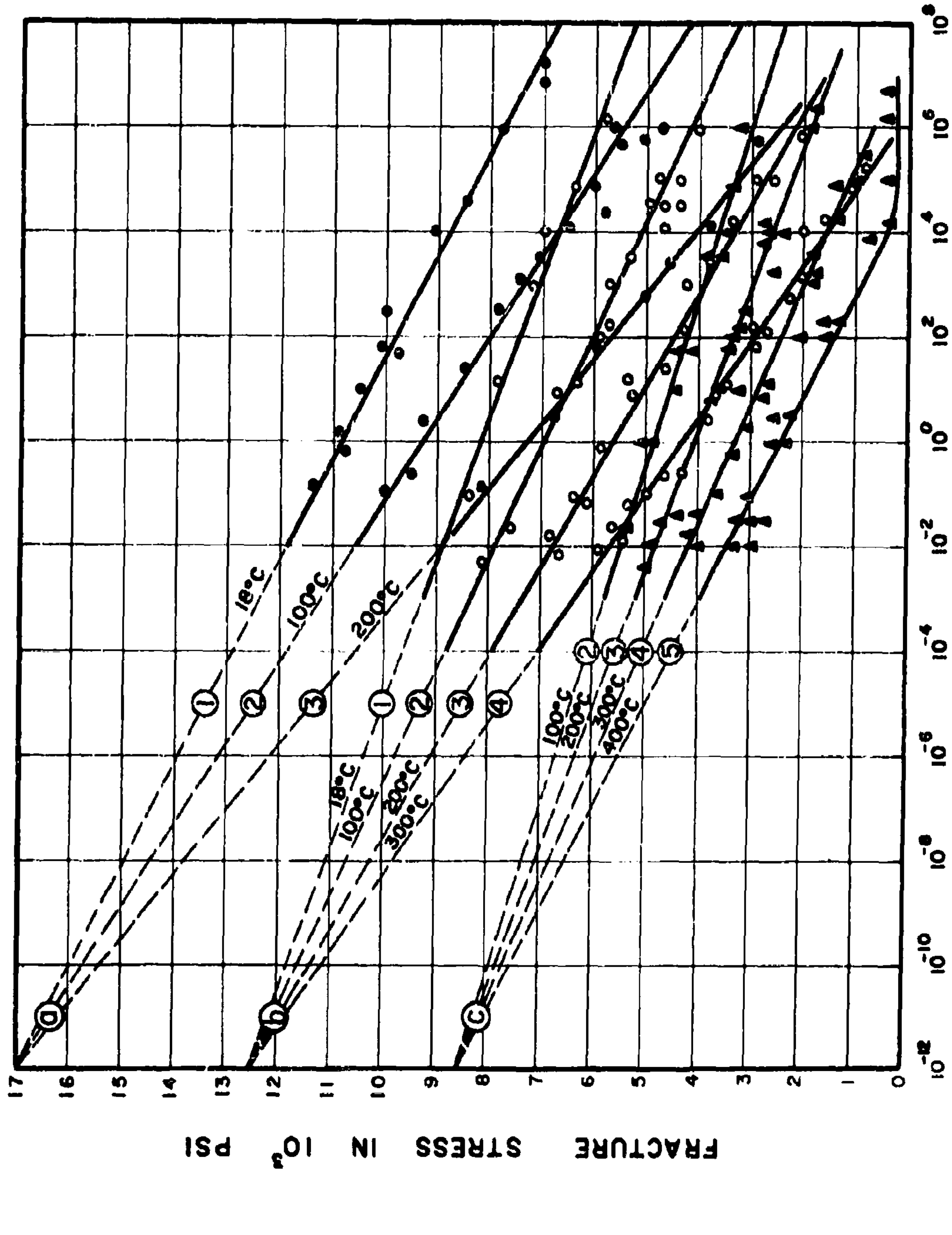

Fig. 3 Time Dependent Fracture Strength of Aluminum (after Zhurkov) Annealed at: a) 290°C; b) 420°C; c) 550°C.

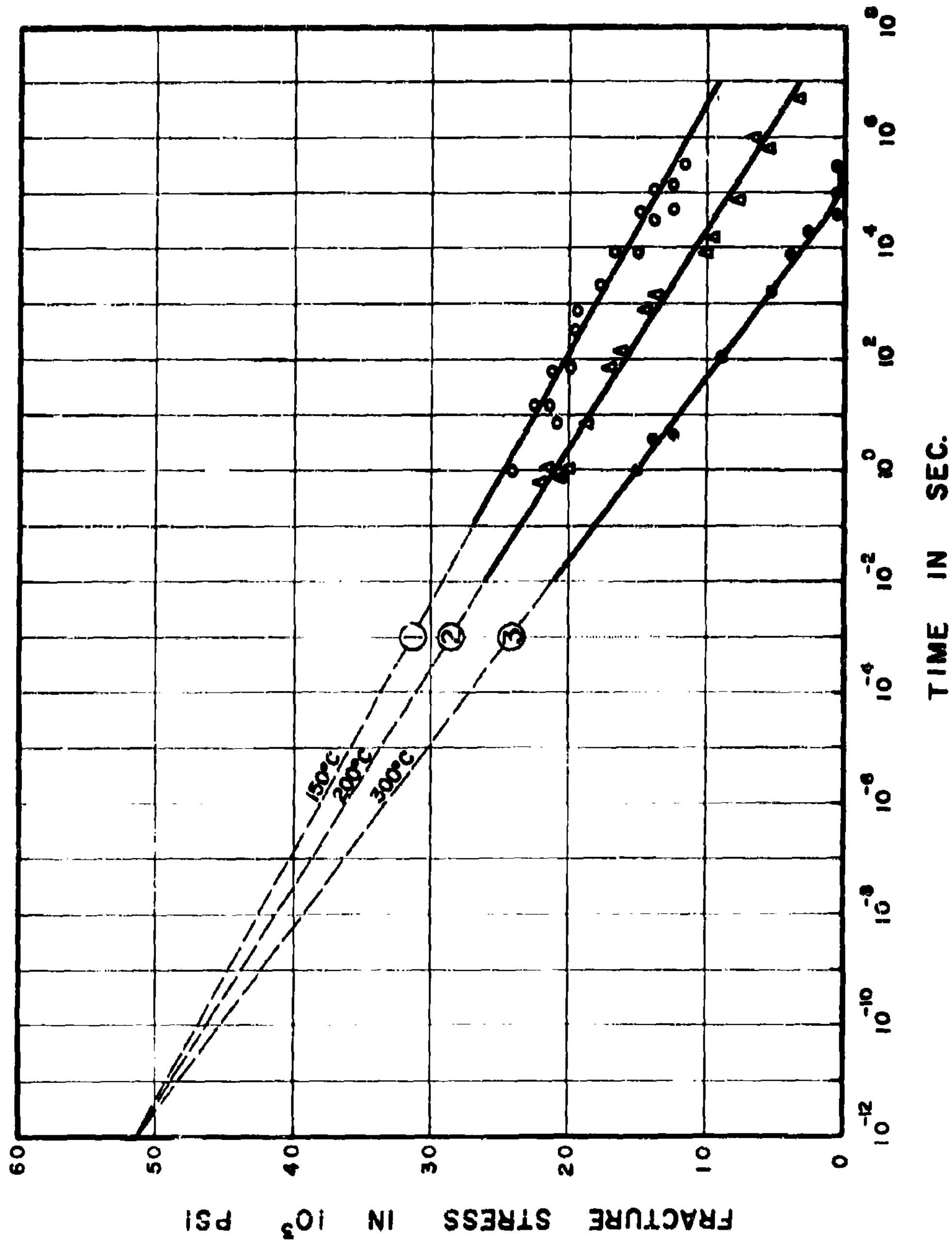

Fig. 4 Time Dependent Fracture of Al+2%Mg Alloy (after Zhurkov) Annealed at 420°C.

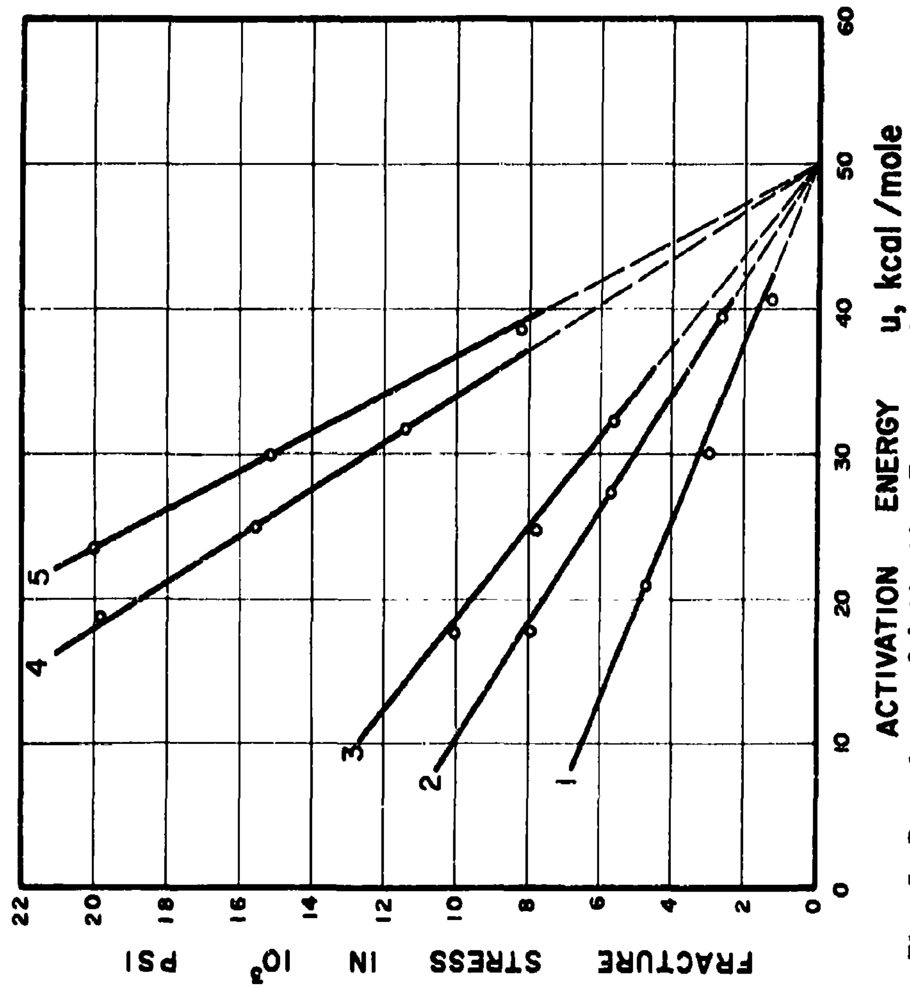

Fig. 5 Dependence of Activation Energy on Applied Stress in Aluminum (after Zhurkov) Annealed at 1) 550°C; 2) 420°C; 3) 290°C; 4) no annealing; 5) Al+2%Mg.

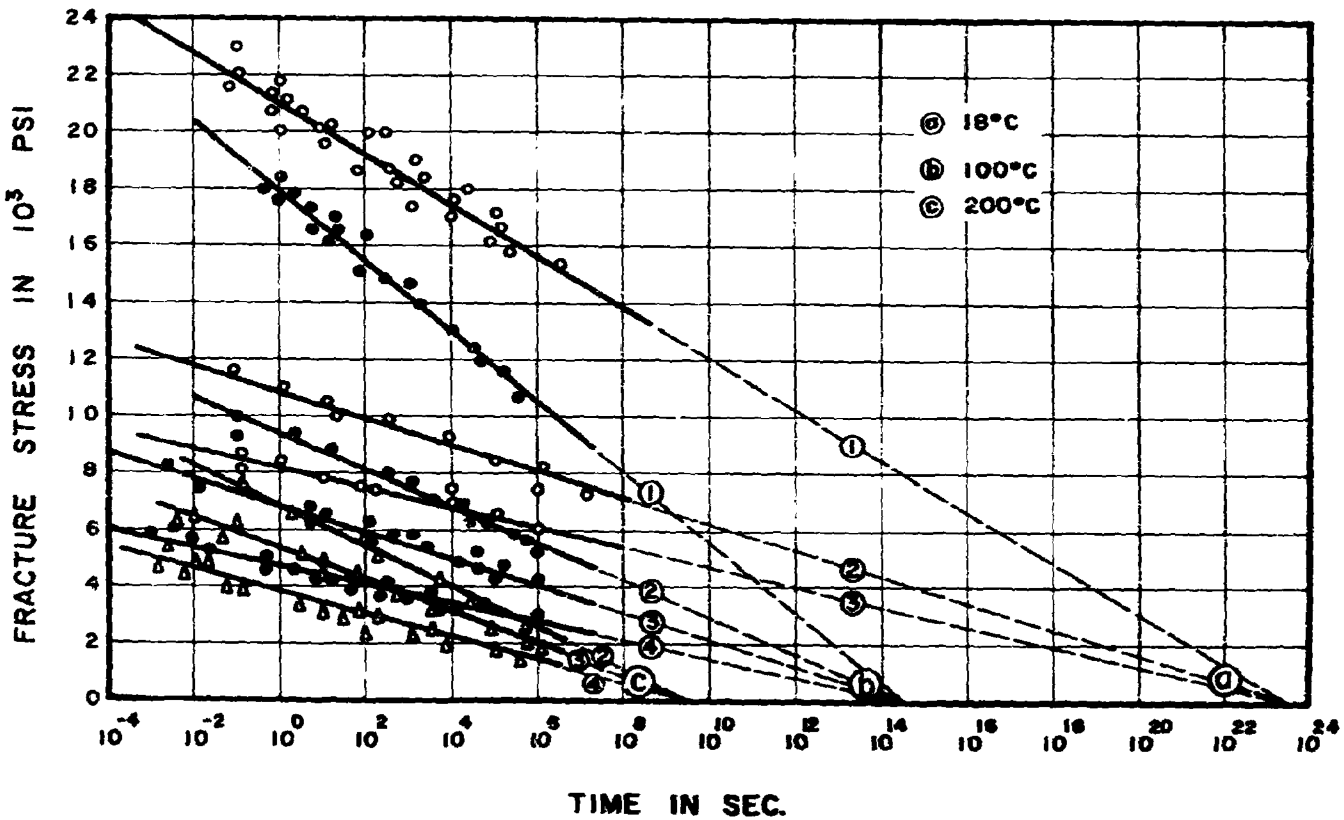

Fig. 6 Time and Temperature Dependent Fracture Strength of Aluminum (after Zhurkov) Annealed at 1) no annealing; 2) 290°C; 3) 420°C; 4) 550°C.

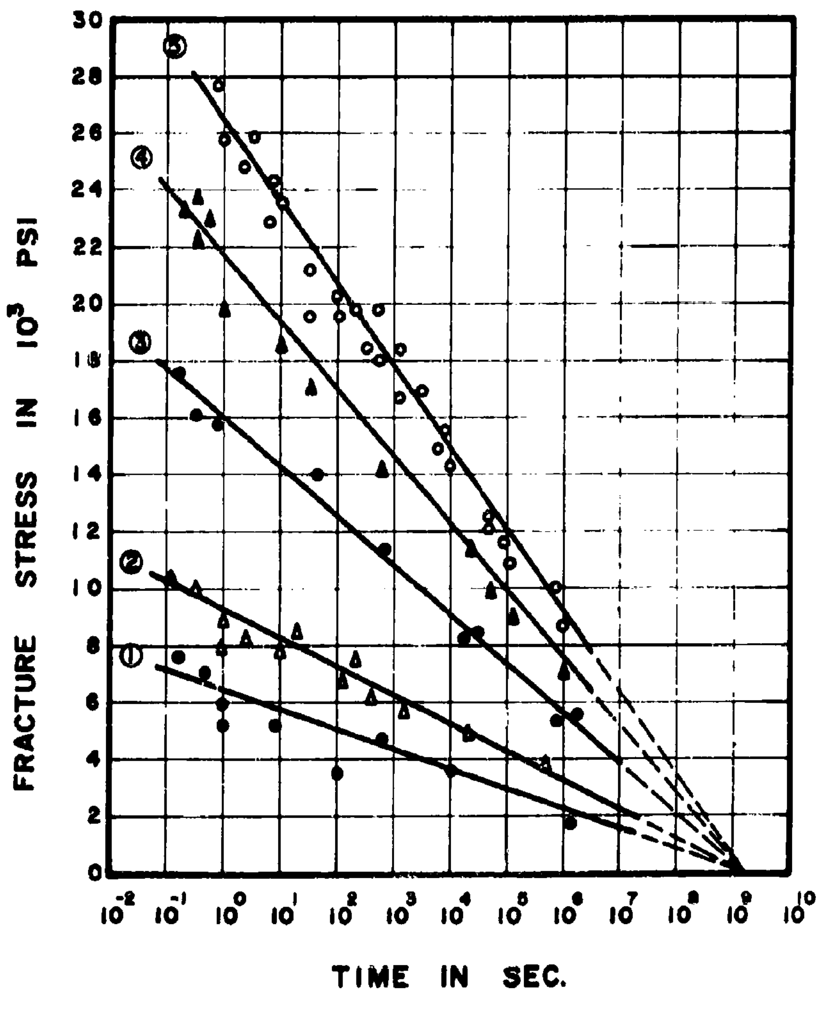

Fig. 7 Time Dependent Fracture Strength for Zinc at Room Temperature (after Zhurkov) Annealed at 1) 360°C; 2) 260°C; 3) 220°C; 4) 180°C; 5) 100°C.

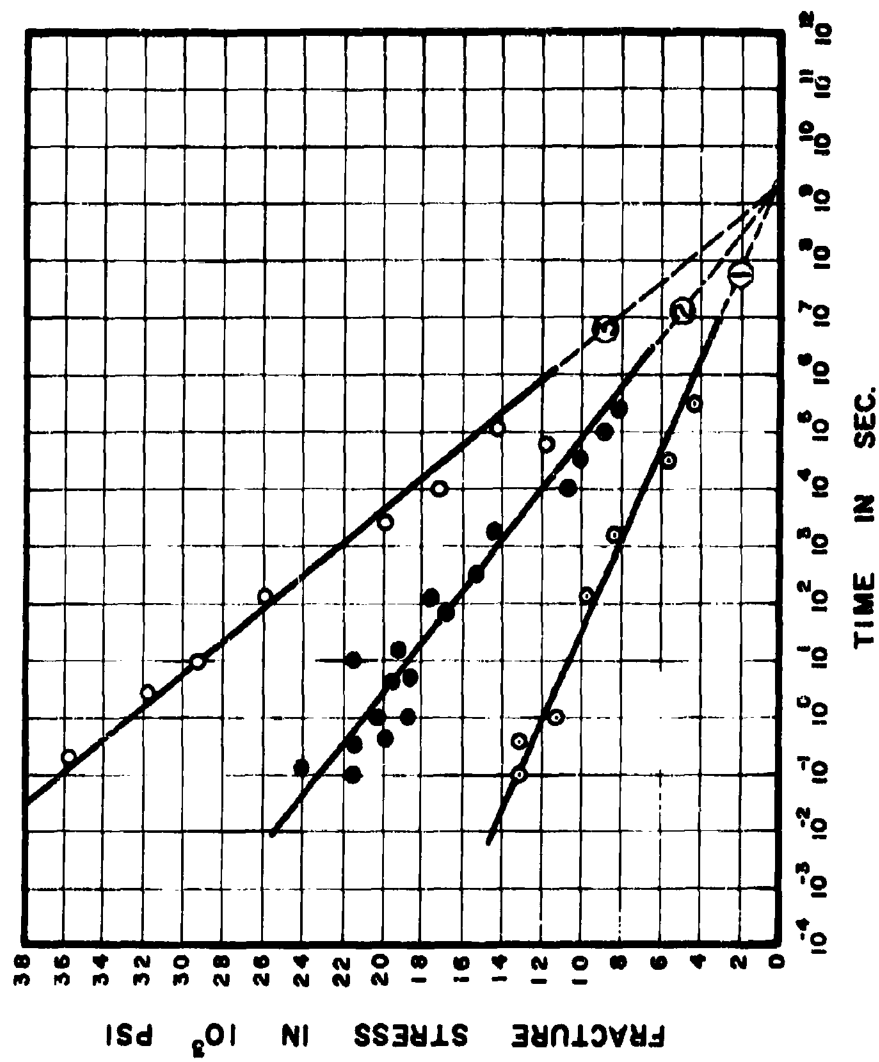

Fig. 8 Time Dependent Fracture Strength for Al+2%Mg at 200°C (after Zhurkov) Annealed at 1) 600°C; 2) 420°C; 3) no annealing.

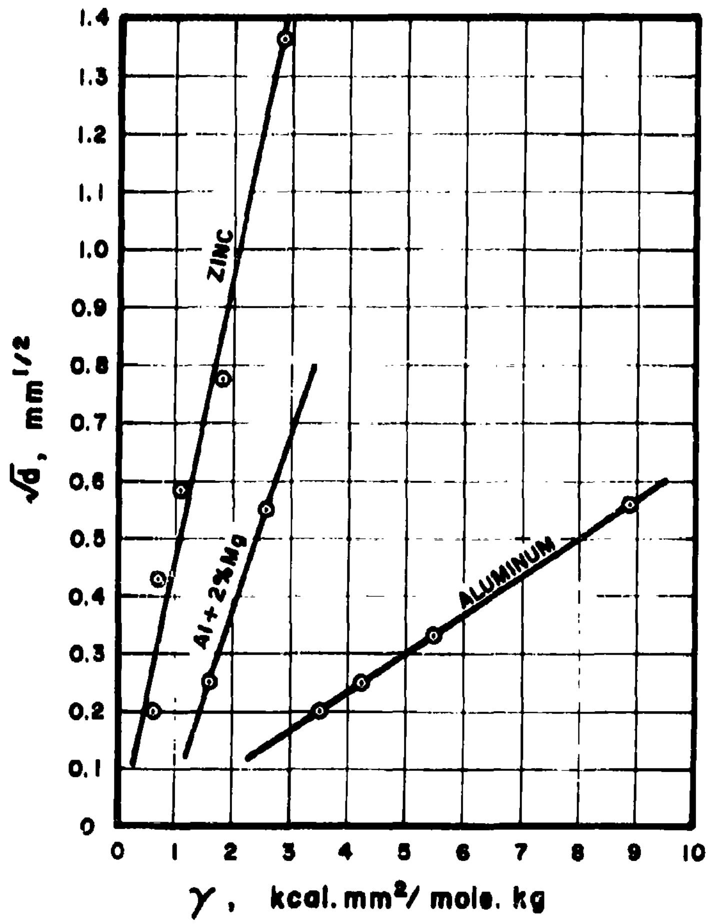

Fig. 9 Dependence of Parameter γ on Grain Size (after Zhurkov)

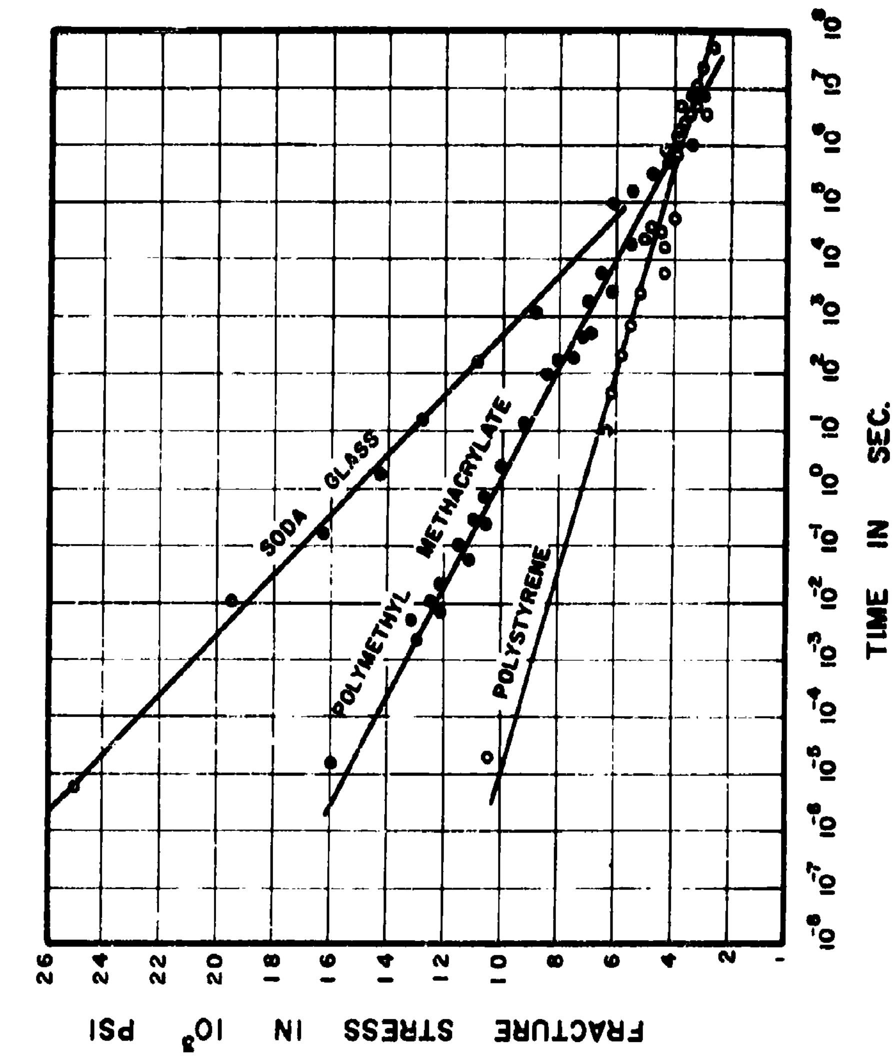

Fig. 10 Time Dependent Fracture Strength of Several Brittle Solids at Room Temperature

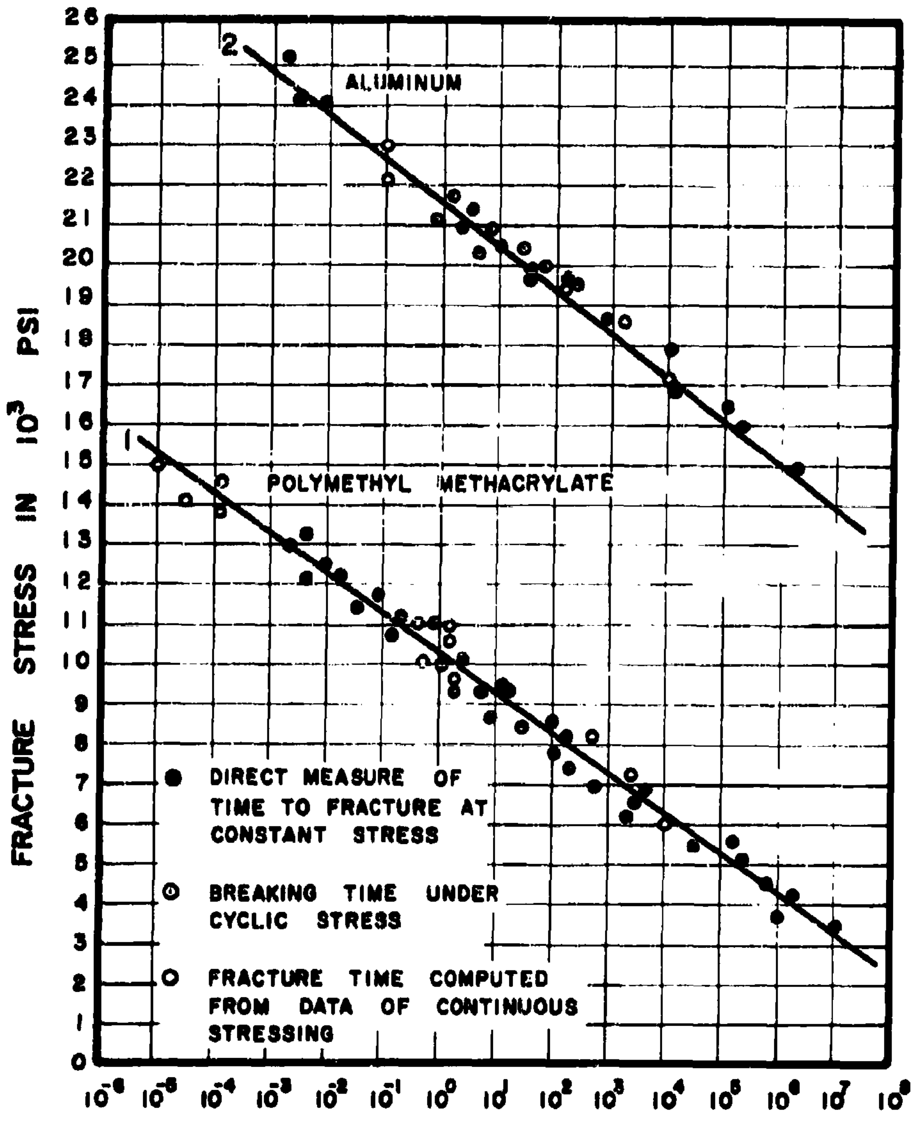

Fig. 11 Time Dependent Fracture Strength of Aluminum and Polymethyl Methacrylate at Room Temperature under Different Stressed Conditions

DISCUSSION

DR. PLASS

We are running a rather close schedule this morning. If there are people who wish to discuss these first two papers, please save your comments for this afternoon's summary session when we will have a general discussion of all the material that was presented at the symposium.

The third and final paper on this morning's program is on "Spall Fracture," and the author is Mr. C. D. Lundergan of Sandia Corporation, Albuquerque, New Mexico. At the present time Mr. Lundergan is Chief of the Physical Properties Division at Sandia. He received his B.S. and M.S. degrees from the University of Notre Dame and has done some graduate work beyond his Master's degree. He has experience as an academic man at St. Louis University, having been the Acting Director of the Department of Aeronautical Engineering at that University for several years in the early 1950s. He also taught at Texas A & M College for two years in the Department of Physics. He has been with Sandia since 1956. Mr. Lundergan.

MR. LUNDERGAN

It seems that the less time it takes to produce these various interactions, the more and more time it takes to talk about them. In this particular paper we will be talking about interactions which occurred in the order of a fraction of a microsecond. In fact we are concerned with the interaction of shock waves in solids and this means that the time of interaction that may produce spall is limited only by the width of the shock waves themselves. We will be concerned with establishing spall thresholds, and by this I mean the minimum tension that is required to produce a particular effect on the material. Further, as Dr. Plass mentioned in the introduction, we will also be very concerned with establishing criteria for the identification of the existence and severity of spall. I think this is going to be a very important item of discussion because this will probably reconcile some of the apparent discrepancies that exist in reported spall thresholds.

SPALL FRACTURE

by

C. Donald Lundergan

Sandia Corporation

SCDC-2845

SPALL FRACTURE

C. Donald Lundergan

Albuquerque, New Mexico

ABSTRACT

The reported spall thresholds in copper (the upper and lower fracture, and the separation) are discussed in relation to the stress-time history in the region of failure and to the experimental techniques used to induce the shock waves. Also considered are the dynamic properties under uniaxial strain, and the geometry of the medium which influences the fracture and separation spall thresholds in polycrystalline metals. Some suggestions are made on using the various approaches of determining spall thresholds to obtain more information about the microscopic and structural behavior of mediums subjected to short time intense impulsive loads.

SPALL FRACTURE

INTRODUCTION

Spallation, the fracture of a medium subjected to shock induced tension, has been investigated with renewed interest the last few years. This interest has been stimulated to a great extent by the exploration of space where the severe environments encountered could provide the necessary physical conditions which would result in the spallation of the structural members of a space vehicle. The objective of a number of these investigations has been to establish design criteria by simulating the expected environments and measuring the response of the medium. In addition to these phenomenological studies, the experimental techniques and instrumentation now available, coupled with the advances in related fields of study, may provide the tools necessary to study the mechanism of spall. However, to what precision and resolution the spall processes may be studied is still speculative.

Whether the object of a study is to establish design criteria or to study the mechanism of spall, the need exists for general agreement on which criteria will be used to identify the existence and severity of spall. The reported spall threshold, that is, the minimum tension necessary to produce spall, is as dependent upon the criteria used to define spall as the stress-field employed to produce it.

In the discussions to follow, consideration is given to the problems encountered in establishing the stress-time history for a medium subjected to uniaxial strain and the response of the medium to the stress. Also, some criteria for the identification of spall are suggested. It is hoped that the discussion will provide some common ground for investigation of what may be called low-pressure spall.

SECTION I: DISCUSSION OF THE SHOCK INDUCED STRESS

The first spalls in metals to be observed and reported by Hopkinson[1] were produced by explosively induced shocks. This continued to be the principle means of producing spall for several years[2,3,4,5]. Of late, plates driven by explosives,[6,7,8] exploding foils,[9,10] or compressed gases[11] have been used by several investigators to produce the necessary conditions to induce spall. Since the shock waves and the resulting particle and free-surface velocities produced by the plate-impact system are more easily described, this system, with a number of simplifying assumptions,

will be used as a starting point for the discussion (Figure 1).

To illustrate the shock wave interactions which result in the production of a tension wave, it is assumed that the medium is subjected to a shock for which the pressure does not exceed the linear portion of the equation of state in either the positive or negative pressure regions. Figure 2 is the position-time diagram of two plates of the same medium and the resulting shock waves. The projectile moves into the target with a velocity U_1, relative to the laboratory reference system and makes contact at time t_0. The compressive shock fronts produced by the impact are transmitted into both the projectile and the target, compressing the medium to a pressure P_1. The particle velocity in the region compressed to the pressure P_1 is $1/2\ U_1$, i.e., one-half of the original projectile velocity. The compressive shock is transmitted to the back surface of the projectile; at time t_1, a relief wave enters the compressed medium. The action of this wave reduces the particle velocity to zero. The same reaction takes place at the free surface, except that, relative to the laboratory, the resulting particle velocity is U_1. The compressed region is, therefore, being reduced to zero pressure by two oppositely directed relief waves, each of which is accelerating the medium through which it passes in opposite directions. Consequently, tension equal in magnitude to P_1 is produced in the plane in which the two relief waves interact. The assumption that the medium can be described by a linear equation of state leads to a shock-like relief wave. If it is further assumed that the thickness of the front is 10^{-7} seconds, the pressure in the medium goes from $+P_1$ to $-P_1$ in a time corresponding to the interaction time of the two shock fronts, i.e., of the order of 10^{-7} seconds. This interaction occurs at time t_3. Providing the medium does not fracture, a tension wave propagates from the plane of interaction into the uncompressed medium. At time t_4, the tension wave reduces the free surface velocity to zero. In the absence of dissipative forces and boundary conditions, the waves would continue to reverberate in the target indefinitely. An experimental determination of the free surface position as a function of time for 6061-T6 aluminum target and projectile for an interface pressure of 5.1 kbar, which is less than the 6.4 kbar Hugoniot elastic limit of the material[12], is shown in Figure 3. This record was obtained by a slanted resistor technique[13,14] and illustrates the close approximation of the experimental results to the predicted results. The times found experimentally correspond to the times of Figure 2. The second compressive wave, which was produced by the interaction of the tension waves, arrived at the free surface at t_5. The rapidity with which the free surface accelerates indicates that both the compressive and rarefaction pressure fronts are traveling essentially as discontinuities.

While the preceding example serves to illustrate the shock interactions leading to the production of tension within the medium, it is not realistic in describing the problems encountered in obtaining the stress-time history of the conditions resulting in spall. Experimental results

indicate that, to produce an optically detectable spall, the initial impact pressure must exceed the elastic region of the medium. For a medium having an equation of state of the form shown in Figure 4, such an impact will induce a two-wave structure consisting of an elastic and a plastic wave. The non-linearity not only increases the number of interactions that must be considered in the analysis to obtain the stress, but tends to reduce the pressure gradients (Figure 5). This effect is easily discernible in the free-surface record (Figure 6) obtained from an experiment identical to the one previously described. The only exception is that, in the second experiment, the initial impact pressure was increased to 10.0 kbar, which exceeds the Hugoniot elastic limit of the material.

Initial impact pressure is often used as an experimental reference rather than the induced tension because the negative equations of state of materials in tension necessary for the calculation of tension are not available. The calculation of spallation stress is further complicated because the wave form is not constant in time[12,15,16]. In fact, the elastic wave pressure gradient and amplitude are functions of the initiating pressure, the metallurgical properties of the medium, and the thickness of the medium traversed. For these reasons, a constitutative equation of state must be employed in the analysis from which the stress-time history is obtained.

SECTION II: THE RESPONSE OF THE MEDIUM TO THE INDUCED STRESS

Now that the physical conditions have been established (the interaction of two relief waves necessary to produce spall) some comments are in order on the response of the material to this stress. These comments will be based on the assumption that there exists within the material a reasonably uniform distribution of sites that will yield when subjected to the stress field described. These sites may be considered to be existing microcracks, or possibly the points at which dislocations are accumulated. The activation of these sites is assumed to be dependent upon the magnitude of the stress applied, the rate of application, and the orientation of the sites with respect to the stress fields.

A minimum activation threshold may exist in the region of the reverse yield stress that must be attained before any effect can be detected. Once this threshold is exceeded, voids appear at the nucleation sites lying in a band where the tension is first applied (Figure 7). These voids act as sources of relief waves which reduce the tension, or possibly the time that the tension is applied to the rest of the material adjacent to this band. The appearance of these voids is the first microscopically observable effect of the induced tension and, as will be discussed in the next section, their existence and size have a correlation to the residual tensile strength of the medium. The initiation of these voids provides a reference for the existence of spall and has been called by Herrmann[17],

the lower spall fracture threshold. These voids will continue to grow, as long as sufficient tension exists, until they attain the proportions necessary to propagate cracks. Ignoring for the moment the influence of the entrapped momentum contained within the material, these cracks will propagate until the band of interaction is laced with cracks and the shock induced tension is relieved (Figures 8 and 9). The medium has not been severed at the end of this process because numerous appendages still exist which hold it together. Since this condition represents the final state of the medium resulting from the initiation and propagation of cracks, this condition can also be used as a reference point. It is called the upper fracture spall threshold[17]. In the foregoing description the width of the band of interaction would be a function of the distribution of the nucleation sites, the properties of the stress field applied, and the rate of growth of the voids.

These two thresholds, the upper and lower, are more important in understanding the mechanism of spall than the more frequently employed threshold which is the complete separation of the medium. The separation threshold is dependent upon the momentum entrapped within the material lying between the spall plane and the free surface. This momentum is, in turn, dependent upon the wave form of the induced stress and the mechanism that results in the incipient spall. The momentum provides the necessary energy to break the appendages by pulling them apart (Figure 10). Further, if care is not taken to avoid boundary conditions, the material surrounding the region of interaction will appreciably retard the complete separation of the material, thereby affecting the determination of the complete spall threshold[18]

For purposes of this discussion, therefore, there are three thresholds considered: the lower and the upper threshold in the incipient phase, and the complete spall threshold. The portion of these processes to which the term time delay refers[4,19,20,21] is somewhat nebulous, but certainly each step is not only time dependent but dependent upon the many parameters previously mentioned.

At this point, some comparisons of the reported spall thresholds in copper may indicate the magnitude of the influence of the several variables discussed. Rinehart[3], using an explosive experimental arrangement in which a plane shock was approximated, obtained a complete separation threshold of -29 kbar; Erkman[8], using explosively induced oblique shock waves, obtained -30 kbar; Smith[22], using a plate impact, obtained a separation threshold of -20 kbar, an upper threshold of -18 kbar, and a lower threshold of -8 kbar. Even though the medium in each case was copper, the geometries, metallurgical properties, and experimental procedures were sufficiently different to account for the variations of results.

The ultimate spall threshold of copper as reported by McQueen and Marsh[6] (> 150 kbar) involves a completely different process and model. In this case the bond strength was the principal concern.

SECTION III: SUGGESTED CRITERIA FOR THE IDENTIFICATION OF SPALL

Considering the numerous difficulties in determining the stress-time history within a medium, it is evident the criteria adopted to identify the existence and severity of spall in structural materials will be considerably different from the criteria employed for those studies designed to study the mechanism of spallation. The criteria used to evaluate the design of a structure should be concerned, not with the spall, per se, but with the reduction of structural integrity. For this reason, the specimen tested should duplicate the original structure and should be subjected to a stress field which simulates, as closely as possible, that which is expected to be encountered. The analysis to determine the extent of damage should depend upon the remaining capabilities of the structure to withstand further test of its strength. The allowable uncertainty in such a determination would accordingly be dependent upon the knowledge of the forces the structure is expected to withstand. Consideration should also be given in the original design to using various techniques, such as impedance mismatches, etc.[23], to reduce the probability of damage by spall. If this approach is not feasible, considerable precaution should be taken in extrapolating the data obtained under one set of conditions to new conditions.

A need exists for a generally recognized standard test for evaluating the bulk response of engineering materials to shock loading. Since the complete separation spall threshold is the most obvious, possibly a test patterned on a target suggested by Barker[24] would provide some design criteria. The target consists of a tapered plug mounted in a holder of the same material (Figure 11). The purpose of the taper is to reduce the influence of the boundary so that the dynamic tensile strength of the material is measured rather than the tensile strength plus the energy required to pull the spall from the surrounding material. The effect of the peripheral boundary on the shape and texture of the spall surface can be seen in Figure 12. The grooved target consisted of a flat circular plate on which a circular groove had been cut. The depth of the groove was equal to the distance from the free-surface to the expected plane of spall. This groove acted as a source of relief waves which destroyed the uniaxial strain condition. The disturbed region did not fracture in spall but had to be pulled apart by the trapped momentum in the spall which accounts for the curvature and the appearance of the fracture about the circumference. The spall from the tapered plug fractured in tension over the whole area and, therefore, did not exhibit these characteristics.

The criteria selected to identify the effects of shock induced tension is, of course, dependent upon the particular effect sought. As has been suggested, a microscopic observation of a target which has been cross-sectioned and carefully etched will permit the detection of voids and the characteristics of the fracture. The residual tensile strength of the medium was determined by Herrmann, et al[17] by preparing tensile specimens from the impacted targets. These specimens were used to detect the upper and lower fracture spall thresholds (Figure 13). It may be possible that the existence of a time required to produce spall could be obtained from tests of this type.

The time delay for spall in lucite was investigated by Keller and Young[25] by controlling the time interval over which the tension was applied. If it is assumed that the shock fronts are constant in time, the time of application of tension can be varied from one experiment to another by varying the thickness of the target plate and impacting plate. Providing the ratio of the plate thicknesses is held constant and initial conditions for each experiment are the same; the only variable that does not scale is time. The Keller-Young experiments failed to indicate any time dependence in lucite.

The dependence of the time to produce spall on both pressure and time of application of tension can be investigated by using a multiplane impacting plate (Figure 14). The technique used by Smith[22] was to machine seven holes of different depths in the back side of the impacting plate. In this situation, the initial pressure produced upon impact of the plate with the target is consistent, however, the time of application of the induced tension is dependent upon the remaining thickness of the impacting plate. The time duration can be varied from a fraction of a microsecond to two microseconds by this method. The cross-section of a copper target impacted by a multiplane plate is shown in Figure 15. The preliminary results of these experiments indicate that the production of voids in copper is dependent upon both time and pressure.

CONCLUSION

The investigation into spallation has been divided into two categories. One category consists of evaluating the susceptibility of a medium to shock loading. It has been suggested, because of the difficulties encountered in calculating the stress-time history of the medium, that material evaluation consists of simulating the expected environment as closely as possible. Comparative tests of materials might possibly be determined using the tapered plug technique, providing the experimental procedures produce consistent desired shock inputs and the criteria for the identification of the spall damage is also consistent.

The second category of investigation has been an examination of the mechanism of spall. Here the study, as might be expected, is dependent upon many allied fields. The computor programs employed to calculate the tension within the medium require a constitutive equation or state of the medium in both compression and tension. The stress field employed should be capable of description throughout its interaction; that is, it should be free of complicating boundary conditions. The criteria used to identify the existence and severity of the effects of shock-induced tension are dependent upon what phase of the effect is being sought.

There have been numerous advances of late in programming, instrumentation, experimentation, and detection, all of which should be of benefit to both portions of the study of spall. This brief discussion has mentioned but a few aspects of spall. For those with greater interest in the subject, there are a number of bibliographies on spall and its allied fields[26,27,28].

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ACKNOWLEDGEMENTS

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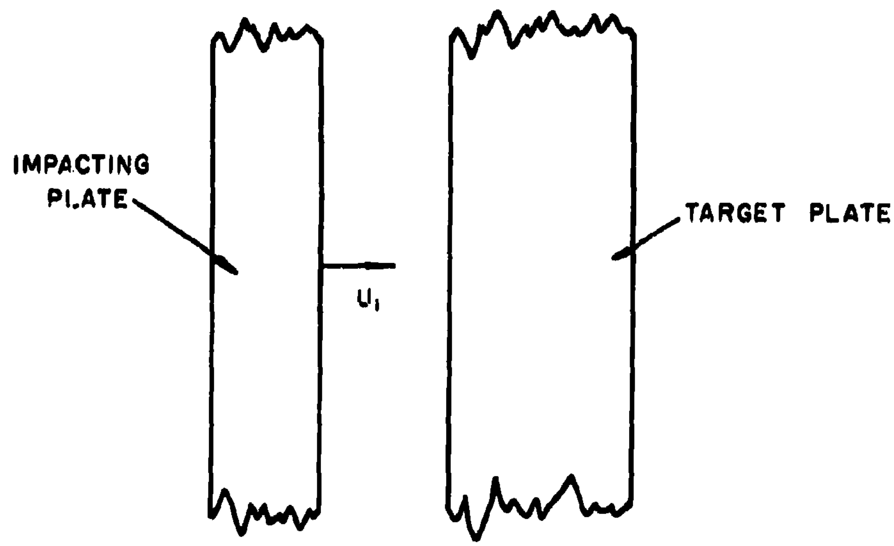

FIGURE 1. SCHEMATIC OF AN IMPACTING PLATE SYSTEM

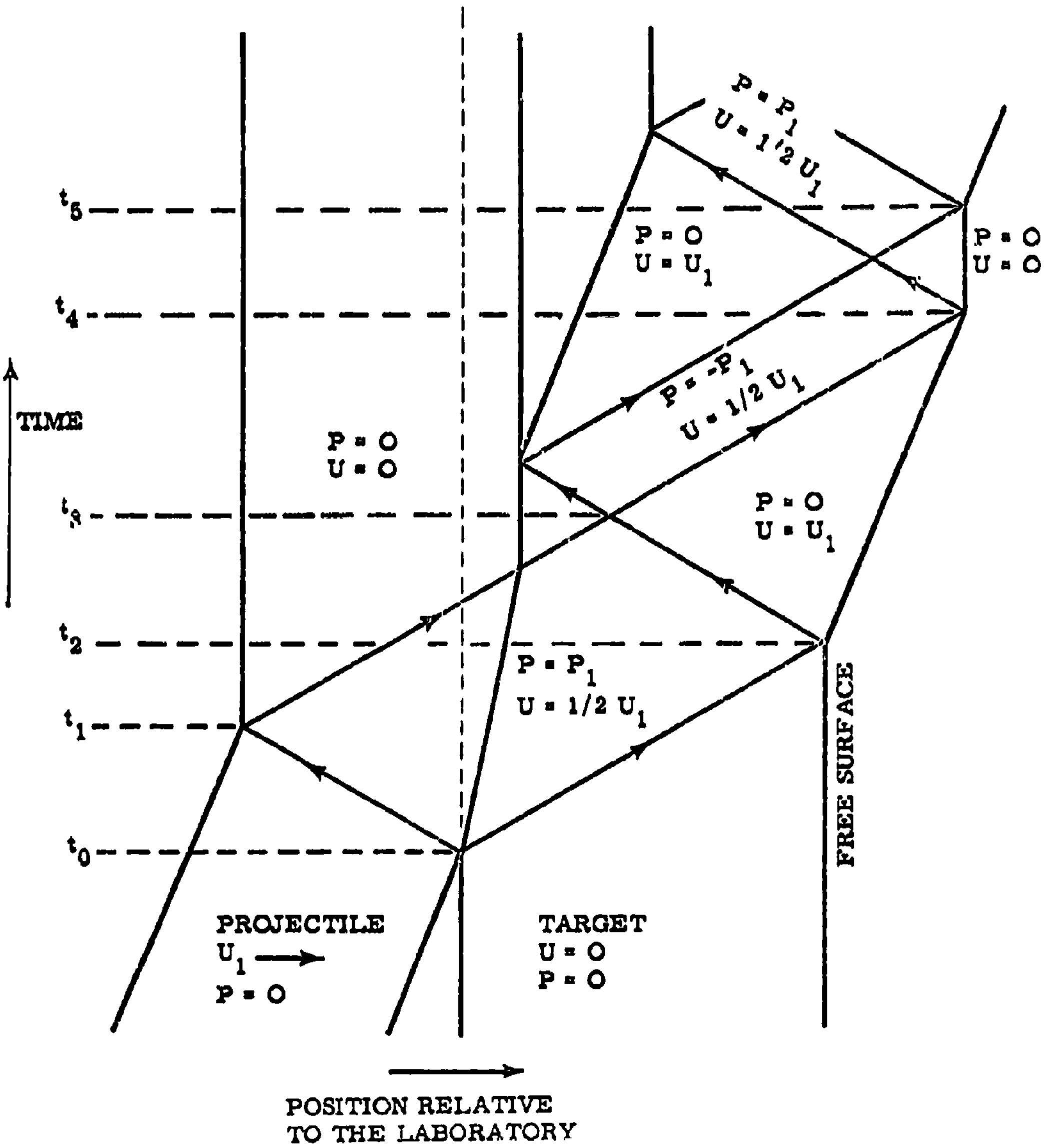

Figure 2. Wave Diagram of a Single Shock Wave Produced by a Low Velocity Impact of Two Plates of the Same Medium

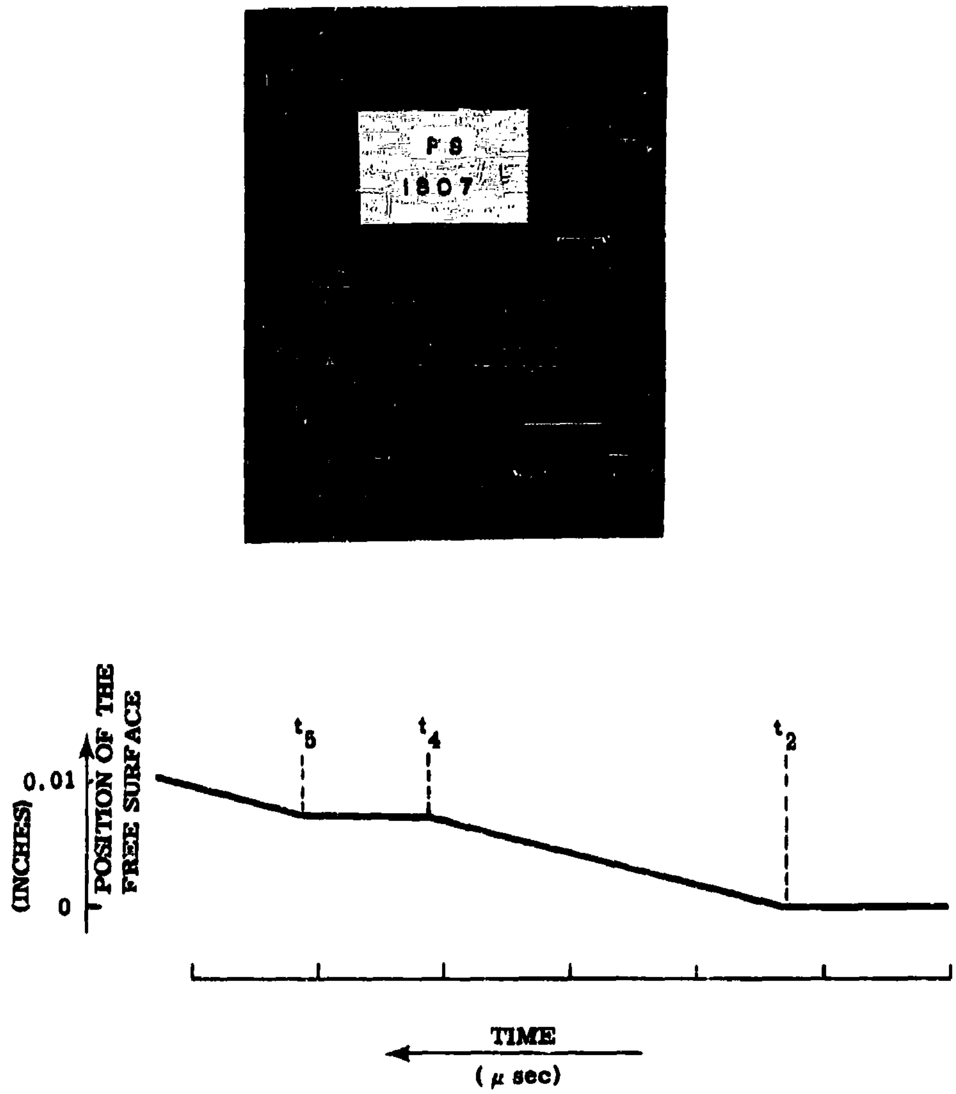

Figure 3 Oscillograph and Drawing of the Position of the Free Surface as a Function of Time

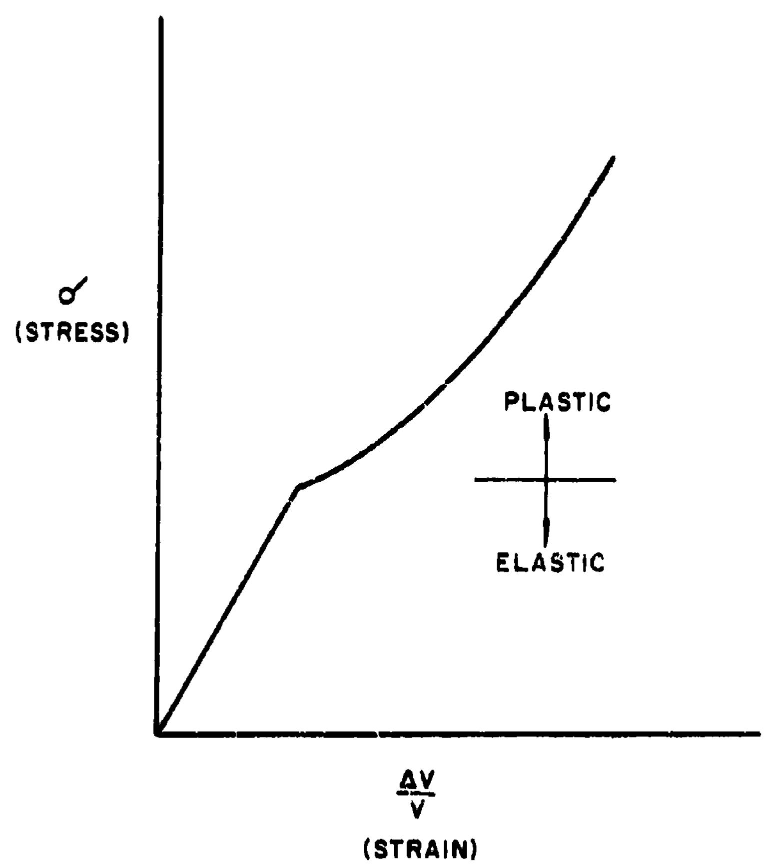

FIGURE 4. LOW PRESSURE STRESS-STRAIN DIAGRAM

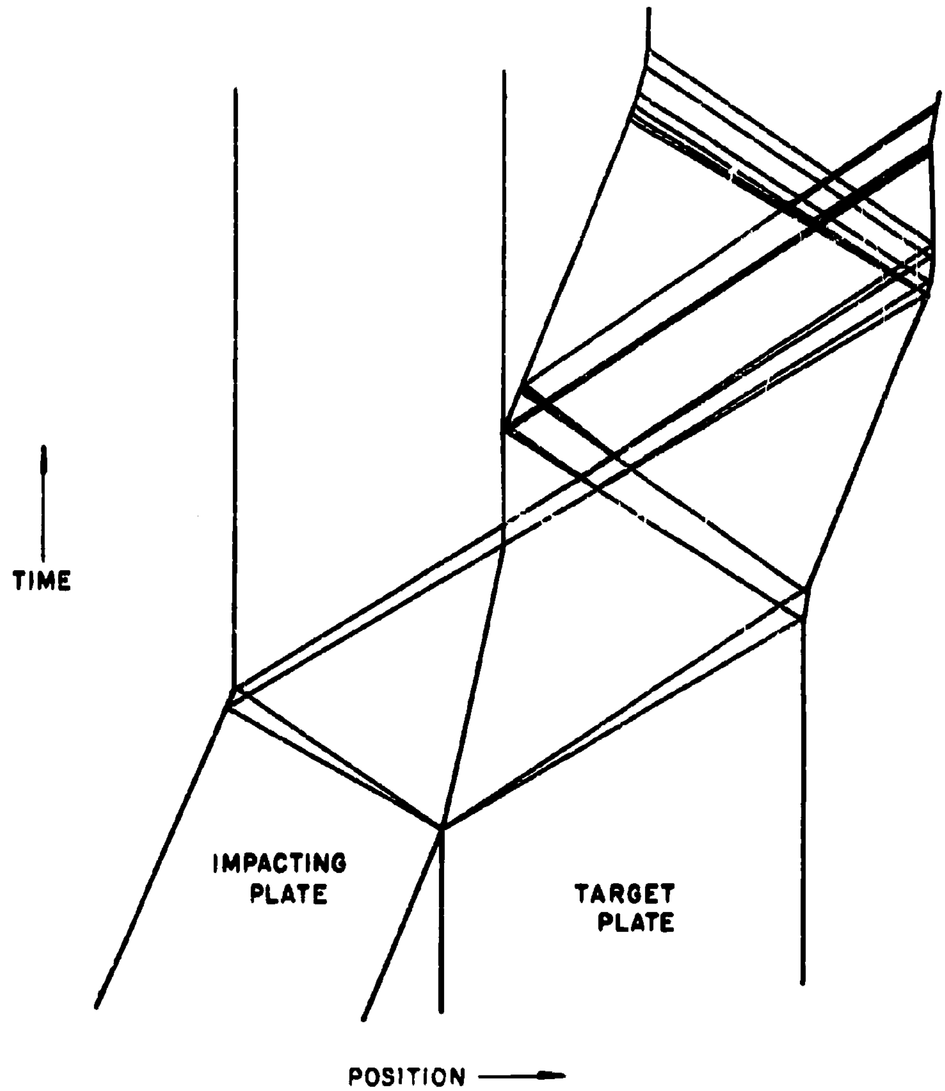

FIGURE 5. SCHEMATIC OF A TWO-WAVE INTERACTION

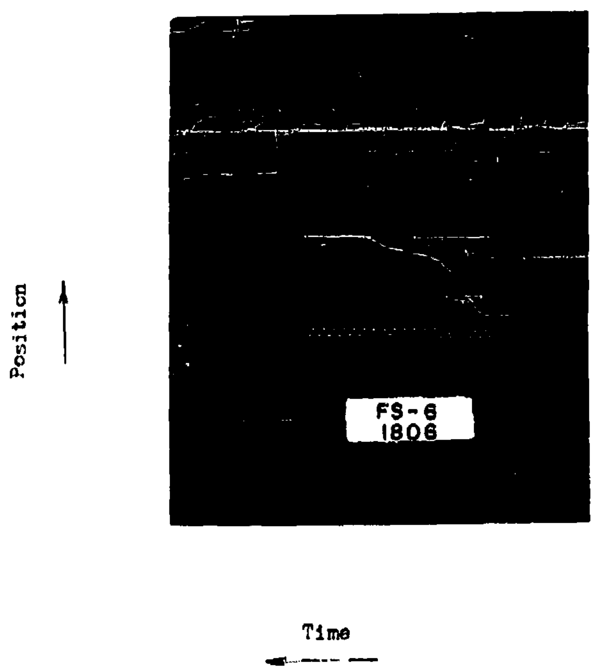

Figure 6 The free-surface position-time record resulting from a two-wave interaction

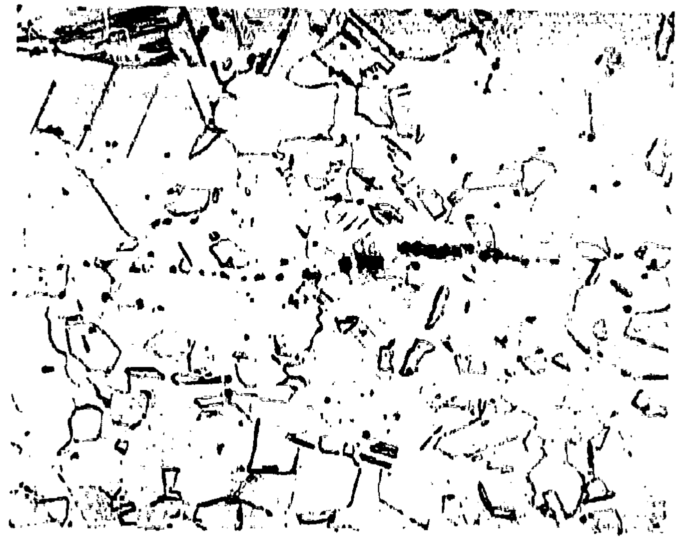

Figure 7 Voids produced in the region of the shock interaction.

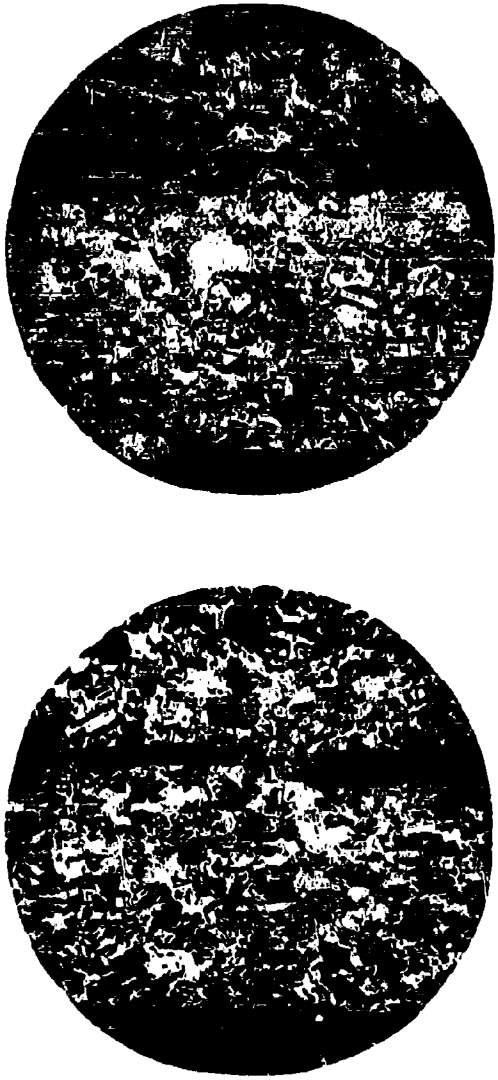

Figure 8 The appearance of the cracks at the upper fracture spall threshold.

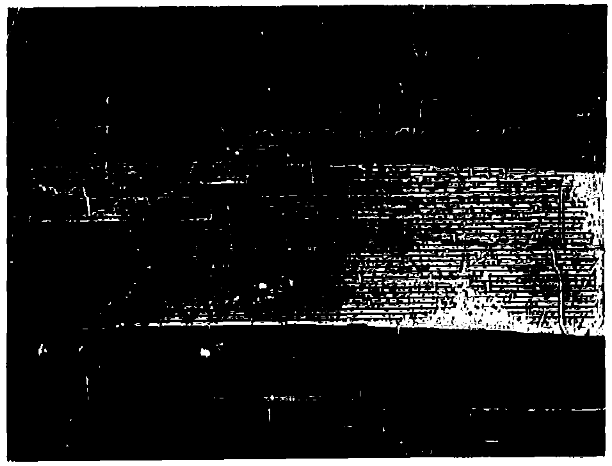

Figure 9. Fracture Spall in Copper

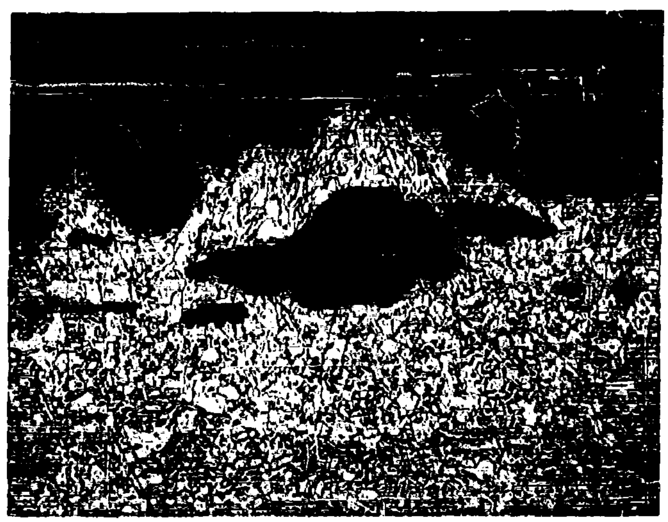

Figure 10. The appearance of the spalled surface after the complete separation of the spalled pieces.

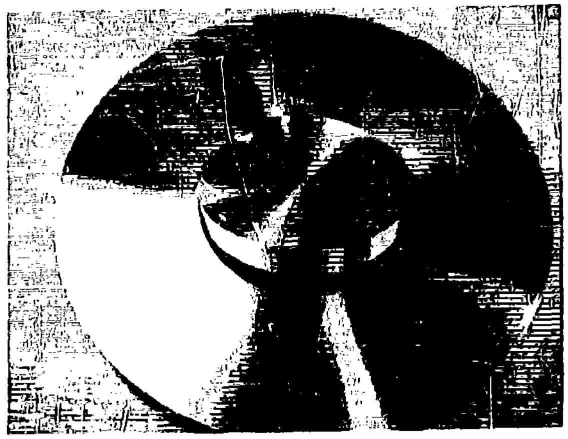

Figure 11. An Aluminum Tapered Plug Target

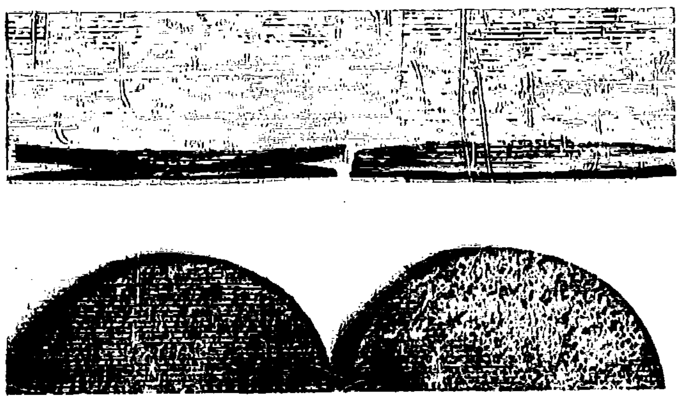

Figure 12. Spalled Piece from a Target with a Circular Groove in Free Surface and a Tapered Plug Target.

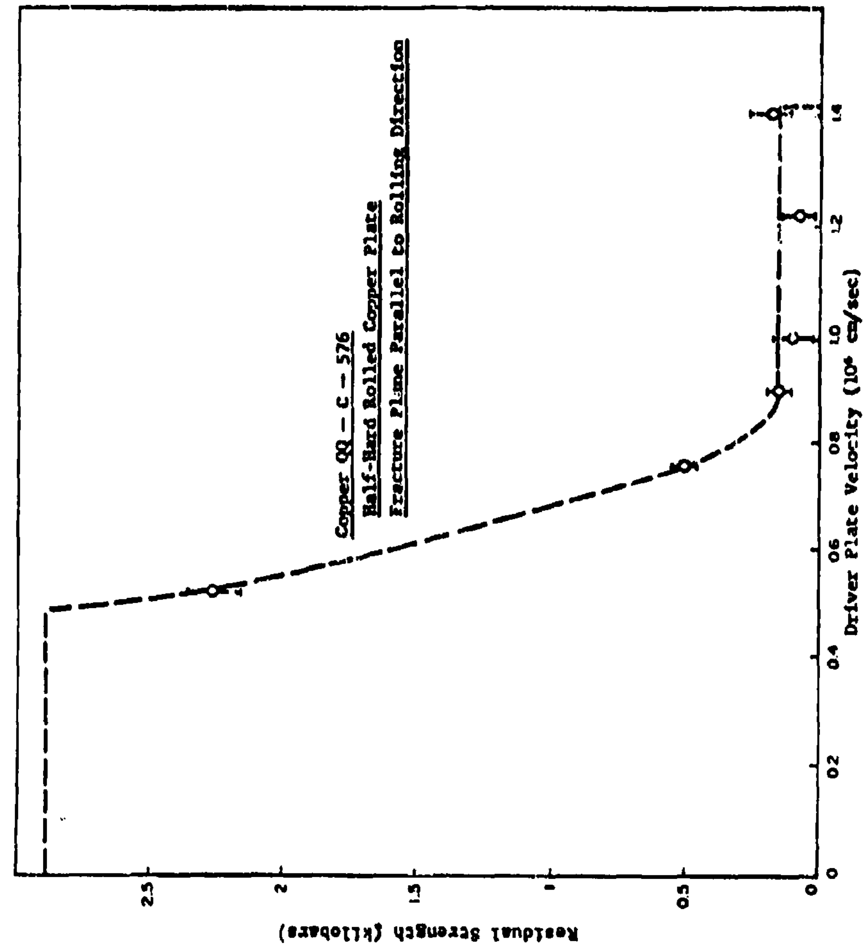

Fig. 13 Residual Strength of Spalled Targets

From Stress Wave Propa[illegible] and Spallation by W. He[illegible], J. A. Witmer, J. H. Percy & A. H. Jones

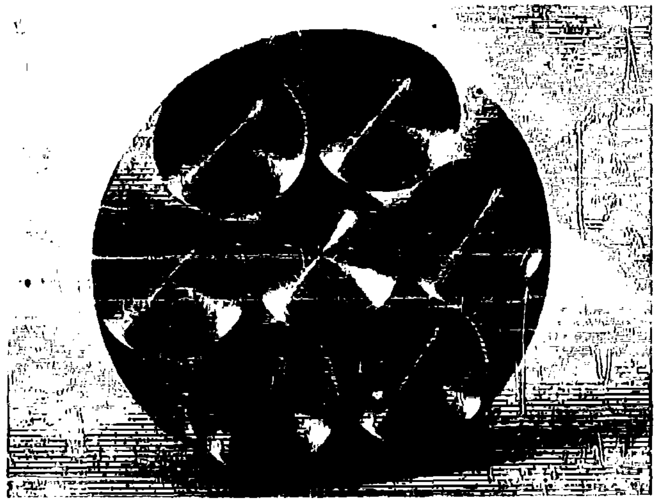

Figure 14. A Multiplane Impacting Plate

Figure 15. Cross Section of Target Impacted By
A Multiplane Target

DISCUSSION

DR. PLASS

We have about five or ten minutes for discussion if somebody would like to ask Mr. Lundergan a question, or has any comments to make about the subject.

FROM THE FLOOR

In the last slide where it showed the positions of the spall occurring in different layers of metal, it wasn't clear to me whether this indicated a time effect, or whether this was just a difference in position.

MR. LUNDERGAN

If it is assumed that the wave form itself is not appreciably changed, we believe if this is the case, it is time dependent.

FROM THE FLOOR

I would like to ask a question about Mr. Lundergan's comment. He did not feel that the plate slap technique was suitable for testing actual vehicles. I wonder if he has any thoughts as to what is suitable?

MR. LUNDERGAN

Well the plate slap technique is suitable for determining damage, but my point was that it is very important to establish what criteria is going to be used to determine damage. If the plate slap technique does produce the impulse to which the re-entry vehicle will be subjected, then it is a very good technique. On the other hand there are other methods, explosive methods, exploding wire technique, for example, which produces a different wave form and if this more suitably simulates the expected impulse, then this technique should be employed. It is one of determining what is expected as far as the damaging load is concerned, and then, finally, after the damage has been inflicted what further loads must the vehicle sustain. So there is quite a broad area of uncertainty.

DR. PLASS

Are there any other questions? Does anybody want to make a comment about either of the two previous papers since we have a few minutes. If not, that concludes our meeting for this morning.

SESSION V

PANEL FOR FUTURE RESEARCH OBJECTIVES

L. R. Standifer, Colonel, USAF
Session Chairman

Aeronautical Systems Division

SESSION V

PANEL FOR FUTURE RESEARCH OBJECTIVES

COLONEL L. R. STANDIFER

The symposium will please come to order.

This afternoon we want to come up with suggested future research objectives that we at ASD can put together and develop something that will be of guidance to us in getting the most out of our applied research program, coordinating of course with our future systems requirements. This is the idea in the work that we have been doing here and has been to a great extent, one of the interfaces, you might say, with the applied research work that we are doing. I would like each session chairman to summarize the work presented by his session and give us his opinions as to the research work that he sees to be done. We will then have a general discussion and ask the audience for suggestions. I would also like the audience to discuss the relation of the session being discussed with the other three sessions. In other words, the inter-reaction of research with the other three sections.

We have changed around some of the sessions to take care of plane times of a few of the principal session chairmen. Our first session will be the Mechanical Properties of Solids. In this case I have interchanged the work of Dr. Dorn and the work on viscoelastic response because they seem to follow better for discussion purposes into the two areas, so in this first session, Dr. Dorn's paper will also be included as part of the properties of materials. Without further ado, I will turn the session over to Dr. Drucker for his summary and recommendations.

DR. DRUCKER

The papers which you heard presented by Dr. Brode, Professor Ericksen, Dr. Stroh and Dr. Dorn really were presented so clearly and they covered so diverse a set of subjects that I felt it would be both presumptuous and rather useless for me to attempt the ordinary type of summary. Instead, what I shall try to do is to convey to you my understanding of their place in the general area of the mechanical properties of solids. Now, this, too, is a very difficult task because each of the speakers aimed at a very different level of basic knowledge and a different level of usefulness. I thought, perhaps, it might be helpful to some of you if I started my analysis with a very simple illustration from plasticity.

Plasticity, in this sense, means time independence, and time independence often is a very useful idealization of the real behavior of material. And, suppose we start with a very simple static structure, a round bar, say of aluminum alloy, whose grain size is negligible in comparison to the dimensions of the bar, and which statistically is isotropic. And imagine we take this bar, pull it into the plastic range, twist it, bend it, and then reverse twist. Can we predict the initial slope of the taut twist curve? Now, to do this very simple kind of problem with just the very moderate degree of accuracy requires a tremendously detailed knowledge of the very complex stress-strain behavior of aluminum alloy. The strain of the tensor is determined not only by the existing state of stress which is a tensile property, and by an increment of stress, also a tensile property, but it is determined also by the entire past history as well. Now, if you add time effects to what I have said and add also the effect of metallurgical structure, then you can begin to see what Dr. Ericksen is talking about in his paper on oriented solids.

Now, many of us are working in this and related areas. A few of the research workers in this country which come to mind immediately are Dr. Onat, Whitman, Truesdale, Budiansky, among, of course, many others. So this, then, is the one extreme where one deals with the very real and very complicated behavior of materials which involve the characterization in a very complex, mathematical manner.

Now, all this real complication in stress-strain relations is unnecessary in a very important area of structural analysis and design. If we consider the same aluminum alloy, not just a little bar which appeared so complicated, but now a really complex structure, we can find the load carrying capacity of this complex structure with very drastic idealizations. The first idealization is a perfectly plastic, nonwork hardening solid. Then the limit theorems of plasticity which were developed by Professors Tyler and Greenberg, coupled with any approximate yield condition you wish which is roughly the yield criterion for the material, you will get a very good answer for the load carrying capacity of the structure. Now, corresponding theorems have been developed for creep by Mr. Calibine of Cambridge, but the general theorems, the time and temperature dependent materials have not yet been developed. So we then have the two extremes: (1) the highly mathematical formulation of stress-strain relations which is essential if we want to solve the very simple problem of this bar I just described; and (2) the very drastic idealization so useful for engineering purposes, if all you want is load carrying capacity. But, in either case, this microscopic or phenomenological formulation does not take into account directly the real nature of the inelastic behavior of materials.

The next level down in scale is the very important microstructural approach, the ordinary metallurgical approach, about which we heard nothing at all at this meeting. This, of course, implies nothing about its relative importance. It is a very important field of study which is very actively pursued by a large group of people. We did pay considerable attention to the next finer scale still, which is called the atomistic and represented by dislocations theory and allied topics. Dr. Dorn, for example, explained very clearly when strain rates do have an effect and when they don't, and gave as an example (he referred to the work of Professor Bell) a face centered cubic metal, an aluminum material, which was annealed so that the number of dislocations per unit of volume was extremely small. Under these and most loading conditions rate effects should be negligible. On the other hand, Dr. Stroh showed us that rate effects would become very important if dislocations were made to travel at supersonic velocities. Now, both the microstructural, which we did not discuss, and the atomistic approach aim at a physical description and understanding of behavioral methods. This, of course, is their virtue, but neither is geared to predict in quantitative terms our tortuous twist relation for the bar. Their technological domain is the design of material, not the design of structures.

We went one step down in level again, in scale, when we looked at the truly atomic level with Dr. Brode. He used a fluid mechanics-thermodynamic approach which is valid under very high temperatures, extremely high pressures, with accompanying high strain rate.

The structure, in a metallurgical sense, no longer is of importance. Sand, rock, or metal, all are treated as fluids; all are given an equation of state. Here, scalar rather than tensor relations are sought, although Dr. Brode said the theory will be much improved if in the later stages of lower pressures, the elastic visco-plastic behavior of materials could be included. But again we must keep in mind the purpose of the various idealizations of materials which one makes. The fluid approach is a sensible first step under these conditions of high pressures and temperatures. Conceptually, I would say it corresponds to the application of limit theorems in ordinary plasticity theory. If I may, I would caution

against falling into the trap of supposing that eventually one or another of these approaches, ranging from the continuum mechanics approach, on the one hand, down to the atomistic approach on the other, will provide the full answer to the entire range, the range that goes from experimental stress analysis (the problem of the bar) through structural and material design, to atomic interactions. Each of our activities has its place: applied mechanics, physical metallurgy, metal physics and atomic physics, and each at the present time is very far from fulfilling its own role when temperature and rate effects both must be taken into account.

Well, the importance of the materials problem is well recognized as indicated perhaps most dramatically by the sponsorship and attendance at this symposium. Materials science now is one of the key magic words, and it should be, both for the engineering and the scientific interest. Our major source of difficulty is that engineering practice has advanced so rapidly, and is continuing to advance even more rapidly, that the store of fundamental information on materials is almost fully utilized, and new information is not being developed fast enough. There are far too few people engaged actively in basic materials research and what is even worse, there are far too few students who are educated in materials science. The Department of Defense is well aware of the growing crisis. ARPA is sponsoring the interdisciplinary materials science centers at a number of universities, in an effort to break the bottleneck as the years progress. This is not a crash program in the sense of immediate response, but a recognition that badly off as we are now, five years from now if nothing were done, we would be far worse off.

Now the scale was very large by old fashioned standards of university research. Our program at Brown University, as an example, which cuts across departments, division of applied mathematics, chemistry, engineering, geology, physics, soon will average over a million dollars a year. Now, other agencies of the Department of Defense were far-sighted enough to anticipate trouble of this type many years ago. ONR, for example, supported extensive work in plasticity at Brown from its earliest days and now supports studies of time and temperature dependence both at Brown and elsewhere very heavily. But generous as this support is and has been, more is needed for basic materials research. I should add, as an aside, that the words you heard from Colonel Standifer were applied research because this is a function of the laboratory, and I am speaking, of course, of the need for basic research to feed applied research which in turn will feed design. Now, the need is especially serious in basic engineering research because engineering is relatively very poorly supported on the basic side because of the high development budgets which in DOD are labeled as research. The costs of basic research rise continually and rapidly, and universities are desperately short of sufficient modern equipment and the accompanying technical staff. A small fraction, an extremely small fraction of the development procurement budget of the Federal Government would be spent very wisely if it were diverted to bring university facilities for basic research to the same level as those of industrial laboratories which are doing applied research. In no area is this need more critical than in materials science and, returning now to ARPA, this is recognized clearly by all, speaking of its contribution as seed money for new projects and new development. The ARPA program is not a support program for existing activity.

Well, speaking in complete generalities, progress is so rapid we must run very hard just to stand still. We must exert almost superhuman effort to get ahead in those areas of materials behavior which we have heard about in the past few days, and in even a larger number of areas we are all familiar with, but which could not be covered in the time available, or which are not in the high impulse category. I think, perhaps, rather than being explicit as I have been in the past on various areas of materials science which are worthy

of developm nt, all one need do is look at the papers which have been presented to you and simply say obviously this work represents directions which must be cultivated, must be expanded. Each of you, I am sure, in turn, can think of many areas of materials research which require intensive basic research as well as applied research. Perhaps, in closing, I should do as Colonel Standifer has suggested, that is, say something about the interaction between the mechanical properties of solids session and the other sessions which we have had.

As I see it, this may of course be a prejudiced point of view, it is the mechanical properties of solids which is the basic area. Only when one knows mechanical properties can one then solve the boundary value problems which I will label as wave propagation; only when one understands material properties can one solve hypervelocity impact problems; and of course, the problem of fracture is just one of the many areas of materials behavior. So, I would say that if one wishes to progress, it will be preferred in two parts, the basic material behavior, and the application of this behavior to the use of both experiments and mathematics to solve particular problems of great interest both to the Air Force and to many other groups as well.

COLONEL STANDIFER

Thank you Dr. Drucker. I wanted to clarify a point in semantics. Actually, when we are speaking of research, I much prefer the term "fundamental" research to the word "basic" research. The Directorate of Materials and Processes, for instance, the organization of which I am the titular head, supports work as fundamental, as Dr. Dorn mentioned here, and I think that some of the work he is doing is supported out of what some people might call applied research money. It is more fundamental really than some of the material presented here. Now to me, something to be applied means, for example, you see a problem out in the future that you don't know how to solve. When you start to examine this problem you find that there is certain fundamental information that you do not know or understand. Now, to me, this is the most proper thing in the world for an applied research man to fund and to pursue. He must have an understanding of the fundamentals so that he can solve a problem; in other words, the old heat and beat approach to metallurgy has gotten just about as far as it is going to go. We are going to have to figure out some way, in my opinion, to pin these dislocations down and keep them from moving at the speed of sound if we are going to keep these materials from coming apart. The first step is to understand what? The dislocation phenomena and the structure phenomena. The next step is how do we apply these to get the properties we want and make the material do as we want it to do. So, the word "basic" research has a poor connotation to me; it means that you don't know why you want to do it. If we don't know why we need the information, I don't see why the Services should fund it. It seems to me it is something some charitable institution should do.

Now, this does not mean that it may not be most fundamental, and maybe much more fundamental than Dr. Kettering's "Why is grass green?" It may be just as fundamental as that, but if we need to know why grass is green, then to me this becomes applied research immediately, particularly in the materials field. And the next step along the road, of course, is one where we begin to worry about putting things together and the interraction in hardware which, again, is applied research; but each area has its own connotation. At the present time, I would like to start with Dr. Goldsmith on my left and come across the panel to see if they have comments on this area before we throw it open to the audience.

DR. GOLDSMITH

The first comment that comes to my mind is to second Dr. Drucker's remark that materials science is the basis for work in the applied areas, such as wave propagation, fracture, stress analysis in general. A fundamental knowledge of the behavior of materials is necessarily a requisite for a complete and adequate solution of problems in these fields. However, we do have two opposing points of view on this subject. One is that of the fundamentalist who attempts to incorporate every conceivable reaction or behavioral mechanism in his equation that purports to describe the material. The other one is that of the scientist who wants to be given the simplest possible equation that he can then use for substitution in the already rather complicated differential equation of motion so that there is hope of solving the problem without tying up the computing facilities of the entire country for a week. These two points of view are, to my mind, still fairly far apart. Mechanisms can be explained by looking at atomistic processes and dislocations. However, one should not presume that these themselves can then be translated into a simple equation containing at most two or three arbitrary parameters, which can be experimentally determined in the laboratory, for use in the appropriate dynamic equations. Both points of view, however, I think are vitally important. We must look ahead 50 years from now and perhaps a mathematical tool can be developed to handle these additional complications which is missing at the present time; we thus require scientists who can look ahead that far and say, "Well, my fourth or fifth order tensor equation will be capable in due time of numerical verification in the laboratory and will then be of use to the more practical-minded individual," if I may call him that. These people are not dreamers; they are perfectly hard realists; they simply must await the fruition of their work, not at the present time, and not five years from now, but sometime in the somewhat longer range future.

COLONEL STANDIFER

Thank you.

DR. PLASS

The only comment that I have that hasn't already been said is that I would like to disagree with Colonel Standifer on the role of the Services spending money on research that he classifies as fundamental and doesn't appear to have a reason for existing for the particular problems they have at hand right now. I think that a certain amount of money should be allowed to leak out of the hole in the pocket someplace, just to let a few people who have intense curiosity about something or other to continue their work. Perhaps this is called "charitable," but I think a certain number of people should be allowed to continue with that kind of endeavor and people who do have the applied point of view should, perhaps, keep an eye on the kinds of results these people get, and if it turns out that their work has application, then begin to make use of it.

COLONEL STANDIFER

I get it back. I think, again, semantics got in the way. I did not say you should not do this kind of work. What I said was you shouldn't do it if you didn't know why you wanted to do it. I do not feel that any laboratory can live long and stay current and progress and anticipate requirements five and ten or fifteen years down the road unless it sponsors--okay, some

basic research. According to my theory, what you are talking about is sponsoring things so that you keep an interface with the scientific community and universities. Now, this is a perfectly good reason for doing it, in my opinion. If a man has something coming along and you have no need or application for it, this doesn't mean you don't do it, particularly if it's in your field and you see an area there. So, I still feel that you should know why you are doing it.

DR. DRUCKER

I was going to suggest, actually, to avoid difficulty, the stenotypist be instructed wherever I said "basic" to substitute "fundamental." I think there is no difficulty really here in philosophy and I suspect that all of us in this room, especially all of us on the panel, were educated along what I would call engineering lines. Everything we do is really motivated, directly or indirectly, but most often directly, by a real need. We are working now, for example, in shell theory, as a group, not because shell theory for some reason happens to be a fascinating mathematical problem. There are many fascinating mathematical problems; many fascinating experimental problems. Everyone who works in shell theory does so for obvious reasons. The shells or things seem to be moving around up in the air, above the air, and don't you want to know what to do about them? And the same thing with materials. Why is materials science all of a sudden the key thing? Not because materials science, basically, is any more interesting from a purely abstract point of view, but because in a sense we have reached now what is called the materials barrier. So, I think all of us in this room, probably, are motivated at least in what I would consider to be the manner which Colonel Standifer would like us to be motivated. We do basically fundamental research in the areas of interest to us, but the areas, for some reason, always turn out to be also the areas of interest to the country as a whole.

COLONEL STANDIFER

Amen. Really, there is one reason why I brought up this question. I heard people say, "You can't do that; that is basic research, not applied research." The man that can draw the line between basic and applied research I have not met. The most fundamental research in the world can be applied research, and this is why I brought the subject up; I know it wasn't part of this panel discussion, but I do think it's important to understand you shouldn't say "I can't do it because it's basic." Is this right? Okay.

DR. BISPLINGHOFF

Without expanding this argument, fundamental versus basic any further, it seems to me that regardless of how we fund this thing, we have to find ways to allow ingenious people to do things which flow from their own imaginations rather than things which are dictated from above. I've been told that the Royal Aircraft Establishment was in a state of chaos right after World War I. By chaos, I mean chaotic management, if any. This was also one of its most productive periods. I think the Griffith biplane resulted largely from this chaotic management. I have also been told that in the depression years the management of the Westinghouse Company was in equal shape and the work on things which flowed from the imagination of people there like Sodenberg and Nadai and others, paved the way for the eventual superiority of this company in turbine development. So I think we must find a way to allow a few people to work on things which they themselves regard as important.

I think I agree with Dr. Drucker's remarks about the fundamental importance of material behavior and the statement that hypervelocity impact is one application of this, once it is known. Certainly, in the problem of hypervelocity impact, the most important key to progress is an understanding of the state equation. This is not to say that we don't have to work awfully hard on the mechanics side to get an answer, but it is the most important physical understanding that is required; that is, an understanding of the state equation as we proceed through different regimes of temperature and pressure.

COLONEL STANDIFER

Thank you, Doctor. In the materials area, ASD is funding work all the way from fundamental work in dislocations and in alloy and bond theory, up to the theory of crack propagation and fatigue. The application of this fundamental work is being carried out by the Structures group of the Directorate of Materials and Processes who is trying to come up with structures, particularly so-called brittle structures, that will survive under the loads we are having. I'd like to now ask anyone in the audience where you think there are specific research objectives that need emphasis along this entire area of materials applications or materials research; where we can see some type of payoff, maybe ten, fifteen years from now; where we should start to build a program and why. Would anyone like to take the first crack?

W. O. DAVIS, HUYCK CORPORATION

As former Chief of the Air Force Office of Scientific Research I have been debating the "fundamental-basic-exploratory-supporting" semantics since 1952 and therefore I intend to say absolutely nothing about it. Apparently the argument has never been resolved and never will be. I would like to suggest a possible new approach which has been indicated from studies that our group has been making, having to do with the time dependence, when solids and structures are loaded. I think that several of the papers at this meeting, and also in the literature, have indicated that when a body is loaded impulsively at high rates, there is a very definite delay in the response of the loaded body to the applied force. Now, taking a, perhaps, philosophically different approach than would normally be taken, one can very easily say that during the period of time before the body has started to respond in its proper manner, during, shall we say, the transient phase, momentum, in terms of the motion of the mass of the body, would probably not be conserved. In other words, a portion of that momentum is stored internally, perhaps in rotating these vectors of Dr. Ericksen's, and perhaps other internal processes, there is nothing serious; but it is internal at the moment. Nonetheless, in terms of external systems it is not observable. Now, we took the approach that one might consider this as essentially a state of virtual momentum. If you consider the implications of this condition, you are led to the conviction (and I think it was brought up in the Huyck meeting too) that you should not ignore the higher order terms in the equation of motion; in particular, the next higher order, and the rate of onset of acceleration.

Now, following more or less the Dirac approach, electrodynamics, one can add a third order term to the equation of motion and explore its consequences and, in fact, in the laboratory, it turns out that this gives a rather simple analytical method for explaining a behavior which would otherwise be quite complicated. In other words, you consider the impacted body as though it were a point, a Newtonian point, but with a delay time associated with it. When you do this, you find you have a very straight-forward way of handling

an otherwise complicated process. The reason I mention it here, we are more concerned with the microscopic behavior and I think that most of us from these various papers concede that there is a delay in the microscopic motion of the body as a whole. However, what is less apparent, there is a similar delay in each volume element of the body and you can't go down to infinitesimal volume element without considering what delay time it must have associated with it. In other words, you can't go to a zero time interval because at some point, at every point, there must be some time delay. Even at the level of the atom, there is the time required for the signal to propagate across the atom at the velocity of light. The question I'm raising is the possibility of a new look at the very fundamental structure of the theory which is underlying this field. I'd like to suggest that one of the consequences of having added the third order term, which has been extremely intriguing to us, is that you immediately raise the possibility of hyperbolic solutions to the equations which previously didn't exist. Turbulence in aerodynamics, for example, falls out very nicely. Things falling apart, and impulsive loading under certain conditions, if you could specify the coefficient properly for the third order term, it should fall out nicely. So I'd like to suggest that this is an area where we feel there is some definite progress to be made, where the very fundamental nature might shed some light on the field.

COLONEL STANDIFER

Thank you. You are suggesting a research program to extend the equations of motion to include the third order, from the third terms, and the time delays and structure relationships?

W. O. DAVIS

No.

COLONEL STANDIFER

Would you state--try to make it one or two sentences--a fairly clearcut definition of the program you would suggest?

W. O. DAVIS

Equations have been worked up by ourselves and by Dr. Minchoni of General Electric. What I am suggesting is an application of this method of analysis to this particular problem we are discussing here today.

COLONEL STANDIFER

I guess this is something that has already been submitted properly to the people here, but would you submit it to Mr. McGrath's people to be included as an idea?

W. O. DAVIS

Don't misunderstand me; I'm not making a specific proposal.

COLONEL STANDIFER

I understand that. Proposal has a bad connotation; your suggestion.

DR. HOPPMANN

I would like to try to amplify what was said and add a comment or two of my own. I don't think we want to be pressed again into the form of enumerating proposals here for people to do because I think this is somewhat beside the point, really. I think all of us want to make progress in this technical or technological science; you choose the words. Generally, we know what we want to make progress in, in a rough sort of a way, and we don't want to waste time trying to set up some abstract definitions of what this is. I take it for granted that all of us have someone to help us in our progress but, and I don't want to make a proposal either, but I want to make a comment on some observations I have made. I can start with Dr. Hsiao's article and any other including my own and make a certain request. I would like to refer to the charts put on the board which showed stress against time. Many times the equations are put down without reference to the experiment, and I think we have gotten into the habit of thinking somehow that these situations are a sort of God-given part of the human mind; everybody understands them; they really have deep and profound meaning.

Now, what I would like to do is to start to question these premises to get the people themselves (1) to talk in such a fashion that the average trained technical man can quickly come to grips with his problem. Many times we just repeat the same old thing, over and over. I have lived long enough now that sometimes I'm shocked when I go back 25 years later and find certain people doing the same old thing, over and over again. That's point 1. I got into some trouble (you can imagine; I usually do) about a year ago when I was probably accidentally invited to a doctoral examination in the area of fluid mechanics and chemistry. Well, God knows, I'm not a chemist, but after it was over, I figured I was asked to be of some help and ask some questions, and so I did. I asked this young fellow did he ever study hydrodynamics. This was inherent, was necessary to what he was doing, but apparently he wasn't burdened with this task and he had not. Then I thought, "This is pretty bad," and, well, I got in deeper and deeper and deeper, but what it came down to was this. The dean says, "It's easy enough for you to criticize; what are you doing about it?" Of course, I had to give some answer to this, and in a knowing sort of way, I said, "Okay, I'll take a look at this problem."

Well, it happened to involve a very mundane thing called viscosimetry and God knows that's drab enough in the sound. You measure viscosity. People have been doing this almost back to the time of Adam, on rotating cylinders, cones and so on. The thing that really disturbed me was, and I think this is a commentary on many of the things we do, I looked closely to see what they were doing and they were plotting their curve with rates of change of strain, and what are the ordinates? How do they get them? The derived unit, and what they were really measuring (and I think I'm inclined to believe you can measure it after a fashion), was the total torque of the instrument, and with a stop watch they could measure the angular speed. So these were the two choice pieces of information they were getting,

but they didn't want this; they wanted to be scientific about it; they had to get the derived quantity which was characteristic of the material. But here, again, I want to raise the question, "Are they really in the long run quantities which are characteristic of the material, in the abstract, independent of form, shape and other quantities?" I think, in some cases, we can get these. Elasticity has been very lucky. With two or three constants, you come a long way mathematically, but when you begin to work around equations of state, the reference axes begin to shift on you; the problem is not nearly so simple. So, I'm still carrying on the battle of the coefficients of viscosity.

The first thing I found out, the first experiment I ran, rotating a cone and looking at it with my open eyes, was that the lines of flow were not like what they had assumed when they worked out the theory, on the basis of which they predicted the coefficient of viscosity. Yet everybody uses it all through the country. Now I'm still probably going to get my head beaten in before I get through with this, but they're going to have to beat it in. That can be extrapolated into many of these areas so the one big thing is the challenge to all and sundry, including myself. We are going to have to look much more carefully at the definitions of these problems. I don't mean as in grammar school, "What is a cat?" "What is a dog?" But, what are we talking about? Perhaps if we will challenge ourselves, maybe we'll make real progress. If we ask ourselves what does that time mean in a critical state of stress? Does nucleation appear and what number of microseconds? What is this thing? And, can we do it independent of the shape and the size and the time elements with respect to the problem. Metallurgists, for years, have disturbed me because they deal with something called metallurgy and they have to pick it up, they have to bend it, and push it or something. We don't worry about this at all. What is the state of stress in the problem when you do this? Why worry about incidentals like this? I'm at a loss to know what this means. I hope I make myself clear with the few examples I have given. I think this is one of the things we should do. We don't have to keep building battleships just a little different, one after the other. We've been doing it for years, and the same way with aircraft.

Now I'll just wind up with a comment, a critical comment on the military. I worked for a Commander that never became an Admiral and graduated first in his class in the Naval Academy. He was a hardworking sincere man, and I remember, one day, listening to him talk about a dual purpose 5-inch gun in the happy days before 1939 and he almost had to eat his words because very shortly after that explosives rained from the sky and the 5-inch gun had no meaning whatsoever.

COLONEL STANDIFER

That reminds me of when I was invited to be the graduate school representative on doctoral dissertation in electrical engineering and electronics. I was the last man on the totem pole, down at the end. I had heard about pole derivations and stuff like that, and it finally got down to me and the only thing I could think about was Ohm's law and so I said, "What is Ohm's law?" The guy looked at me and he hasn't to this day answered. He had gotten so advanced that he didn't remember Ohm's law, and I think maybe this is a parody on what you are saying. We should take a look at the things we can see right in front of us and try to tie them in.

There is another area I would like to see discussed, if someone feels he is a good authority on it. I feel that this stuff we're talking about, we're talking of measuring, talking about getting into areas way beyond the microsecond, even a millionth of a second, shape, and I see mass measurements, and I see Heisenberg's principle being violated all over the

place. To me, this field of measurement and instrumentation and dependability on what we're looking at is getting real critical as our theories begin to get better. Would someone like to give a comment on ways of improving the instrumentation and measurements through a program for structures so we can depend on what we're looking at? Would anyone like to make a comment on that? Dr. Goldsmith?

DR. GOLDSMITH

No, I wouldn't, but I would like to make a comment on something else, if I could.

COLONEL STANDIFER

Since there are no comments on that, maybe everyone is as floored by it as I am. I still feel there are areas where we could probably sponsor some more research, just in the area of measurements of these things that we're talking about in materials.

PROFESSOR SYMONDS, BROWN UNIVERSITY

Perhaps this will give the people who should be answering your question time to think of the proper thing to say.

Well, I came here because the symposium was labelled Structural Response. This appears in the title somewhere and this is the field of interest. I'm very much impressed by the amount of work that has been done, the amount of knowledge that has been gained of a very fundamental sort, but I must confess that I haven't heard as much as I would have liked to hear about structural response. This is structures of the ordinary sort such as beams, plates, rings, shells of various sorts and combinations of these, where Newton's laws are still obeyed. Professor Hoppmann gave a very nice resume of certain aspects of structural response to high impulse loading. I don't think he quite did justice to the range of response in which plastic deformations play a very important part. He concentrated pretty heavily on situations where solutions in terms of normal modes would work. Well, as soon as you have plastic deformation coming in, normal modes just won't work. This has been obvious because the equations immediately become highly nonlinear; boundary conditions are nonlinear, time dependent, and in general, things are very tough. I would just like to say, if I'm not too presumptuous, what I think are the most serious difficulties in the way of analyzing structures. This is as structures, not penetration problems or things like that, but simply to find the final deformations of structures under high-intensity time dependent loads. So the difficulty seems to me to come from three sources. One is the fact that the analysis must take account of very large changes in geometry. The other is the fact that the analysis must take account of drastic changes in the materials behavior. These are problems of analysis essentially. The third major difficulty, of course, is the difficulty of expressing, of knowing what the material properties themselves are. As far as analysis is concerned, as you know, many, many of you here have worked on problems of this sort. It's very difficult to solve, to overcome these difficulties that I have mentioned, and which you run into immediately when you try to deal with complicated structures, that is, submarines, airplanes, whatever. As far as analysis is concerned, one can achieve a tremendous simplification by separating the plastic and elastic effects. One can approach these problems under the assumption that elastic deformations are negligible. This approaches the limit when your Young's modulus and other elastic moduli approach infinity. This is

plastic approach. This is a sensible thing to do; nobody understands what the limitations of this approach are. It's not just a matter of having large deformations. If you have large deformations it will take a long time to produce them. Elastic effects will certainly be important. One cannot leave them out of the analysis. In general, I think this is a good starting point. One must be able to look at these other complications, but it seems clear to me that analytical approaches would not work. One must have numerical approaches and my own feeling is that a lot of work is needed in studying numerical approaches that are capable (that is the simplest reasonable numerical approach), of overcoming these major difficulties I mentioned, large geometry changes of changing material properties. Thank you.

COLONEL STANDIFER

Thank you, Doctor. We have about four more minutes. Are there any other comments, either from the panel or the audience?

DR. DRUCKER

Yes.

COLONEL STANDIFER

Dr. Drucker would like to respond.

DR. DRUCKER

This business of order, order of terms in the equation was discussed very intensively at the symposium in Haifa on second order effects in various materials. Now this Professor Truesdell made it quite clear that order, of course, was simply what you defined order to be, but whether its first, second, or third order depends entirely on your point of view, what you happen to decide on the fundamental equation on which you deal with fundamental constitutive relations. I think there is a grave danger in this approach, using order as your base because as was pointed out again at the symposium, if you take a material which for simplicity, call a Kelvin type material and you go first, second, third, fourth, fifth order and so on, you never come up with a Maxwell material and conversely, if you start with Maxwell material, go first, second, third and so on, you never get to a Kelvin material. So you can spend your lifetime and keep getting closer and closer approximation but essentially what you need is the infinite range the end of which you never approach. So I think it can be a very dangerous thing to do without a really good fundamental theory from which order then is derived.

I would second Professor Hoppmann's comment that when one needs basic materials characteristics, it is again a matter of some extent of the order. I think, even though you define very clearly what it is you are measuring, why you are measuring it and of course, as Colonel Standifer said, you must be able to measure what you think you will measure, it is a very difficult undertaking. I certainly would second his suggestion that a great deal of work be done in experimental techniques because all too often, if one makes a small number of measurements with poor experimental techniques, especially with preconceived

notions of what the answer ought to be and it's plotted on log log paper, then of course you come out with the answer. I think Professor Symonds' comments are extremely important if one is interested in structures. One must, of course, decide on what are the very simplest developments you can put in to get an answer, and then, of course, it is obvious that one needs numerical techniques to solve the problems. Analytical techniques are really completely out, even for the very simplest of structures, much less the kind of structures of interest to the Armed Services.

COLONEL STANDIFER

Our allotted time for the materials portion of the discussion has passed, but I do want anyone here who upon going home and thinking about this for a while, feels there are areas that are in dire need of planning for long term research particularly, to send in your ideas; write them down--they don't have to be formal. Send them to Mr. F. J. Janik, Chairman, Technical Committee, ASRMDS, Wright-Patterson Air Force Base, Ohio.

I know that you are going to have thoughts when you get home. We are going to publish a summary of the ideas discussed in this session. I don't know if it will be for general distribution, but we would love to have your ideas if we do not have time to discuss them during the sessions. This is not for materials only, but applies to each of the other sessions. Put them down informally and send them to Mr. Janik, and we will appreciate it. I would like now to go to our next session which is on hypervelocity impact. Dr. Bisplinghoff.

DR. BISPLINGHOFF

I suspect that each of us has taken a different approach to our responsibilities of formulating a national program with future research objectives. The way I tackled this problem was to bring together in a caucus the three speakers from last night, Dr. Bjork, Dr. Herrmann and Mr. Curtis, and we attempted to lay down a few recommendations and suggestions which we think are worthy of passing on to the Air Force. I don't think the results of this caucus could be labeled a national program, certainly, perhaps not even an Air Force program or even a program. As advisors or consultants, we really speak from a position of irresponsibility and what we have to say must be examined carefully by the Air Force, first to see whether they are already doing it, which I'm sure they may be in some cases, or whether it merits doing at all. I imagine many of you who are here from the universities are consultants of one kind or another and you may have heard the story about the Ohio farmer that had a flock of pullets and they were lying down on the job of egg production, and he tried everything he could to increase their egg production. He lit the house up at night and he played soft music, but to no avail. His neighbor had a flock of pullets also, and each pullet laid one egg a day, the maximum possible production. So the first neighbor went over to the second and asked what he did to get such good production, and he said, "Well, I have a parrot that I put in my henhouse every night and the parrot repeats, 'lay more eggs, lay more eggs,' and the chickens respond beautifully." So the first farmer asked him if he could borrow this parrot and he did. He took it back to the henhouse and left it there overnight. He came out the next morning and found that the cockerels had the parrot backed up against the wall and the parrot was exclaiming wildly, "Don't get me wrong, gentlemen, I'm just here in a consulting capacity."

Well, my remarks as a consultant are divided into two general areas of, first, theory and experiment, and second, projection techniques. This is more or less according to the way

the papers went last night. In commencing the remarks on theory and experiment, we want to emphasize very strongly the importance of a close coupling between the experimental facilities on the one hand and the theoretical capability on the other. We note with dismay that in many cases the theoretical capability exists in one organization and the experimental facility in another. We feel that progress can only be made by a hand in glove collaboration between these two capabilities, and we would like to stress very strongly that future planning be made in a way so that there is a close proximity, physical proximity, between the two groups. By theoretical capability, we refer, of course, to a heterogeneous collection of disciplines, metallurgy, mechanics and metal physics. With regard to theory, as Dr. Drucker has indicated, the state equation is the key to progress in this understanding of the theoretical mechanism of hypervelocity impact. We feel much more emphasis is needed on deducing state equations from experiments on materials. We feel that novel ideas are needed; more imagination is needed. We must go beyond the uniaxially loaded specimen, and we feel that the Air Force should encourage as much work as possible in this direction. It is also evident to us that thermodynamics must be brought to play here to a much greater extent than it has in the past.

With regard to experiments which go beyond uniaxial specimens, we feel, for example, that a study of the continuum mechanics in the vicinity of the crater, the crater being produced by hypervelocity pellets, and a comparison with theories such as those of Bjork represents a fruitful direction of research. We recognize, of course, that our ability to look within a material and measure the responses of interest is woefully inadequate. We urge the Air Force to exploit these techniques wherever and whenever they may be found. We speak, for example, of such things as the work of Fraser, embedding wires in wax targets in which the wires move through externally applied magnetic fields and generate electromotive forces. We strongly urge increased level of exploitation of truly rational theoretical approaches, again, such as that of the hydrodynamic theory. We would remind the Air Force that such research is not just academic, just an academic exercise, but promises much in the way of payoff in the immediate future. We are speaking here, for example, of the further exploitation of the hydrodynamic theory by applying it to materials other than iron and aluminum. We refer also to a matching of the hydrodynamic model with a plastic model, and with an elastic-plastic model, and finally an elastic model as the shock wave progresses beyond the crater boundary. We note in this connection the bearing of this suggestion on the problems of underground shock in nuclear explosions. A strong feeling was expressed by the group that we should always continue our search for scaling laws which will ultimately permit one to extrapolate given test data, for the different materials and different configurations. This is of course a very difficult objective but one which should nevertheless always be kept in mind.

With regard to types of experiments which may be conducted, the committee took note of two general categories: (1) experiments to check and provide guidelines for specific theories such as the hydrodynamic theory; and (2) ad hoc experiments to develop and improve given structural configurations. With regard to the latter type of experiments, we urge that attempts be made wherever possible to go beyond simple go, no-go tests and to measure enough data to construct conceptual models which attempt to explain the mechanism of behavior. We believe that such models may have much value in reducing the cost of these development programs. Other suggestions for experimental theoretical attacks were: (1) More detailed studies of plastic targets. Plastic targets do not, for example, behave in accordance with the hydrodynamic model, (2) Projectile shape defects. This may be of particular importance in air defense; (3) Oblique impact and projectile size effects; (4) Spallation was the subject of one of the other sessions. We would like to mention this subject as one which merits considerable research because spallation may often

determine the ballistic limits of a target which is otherwise capable of containing an impacting projectile, and therefore has a very strong bearing on the subject of hypervelocity impact. The committee noted that a great majority of the work on constitutive equations in the past has been done at Los Alamos and Livermore. We recommend that the Air Force examine the work of these labs in the light of their own Air Force interest and in the light of the common interest of the three organizations, and initiate work wherever necessary to fill the gap which is not now being done. We also noted with dismay that the Russian work on state equations appears to have gone to an order of magnitude beyond ours in pressures. We don't know quite what to do about this except perhaps to recommend that we improve our intelligence efforts. (Laughter)

With respect to projection techniques, we took note of the fact that a great deal of money has been spent in this area and the most novel ideas have been followed through to a satisfactory conclusion and that very few of them have lacked sufficient funds for their exploitation. We believe that the future lies in a concurrent development of light gas gun technique and explosive techniques. With regard to light gas guns, we feel that a continued, orderly, even if small, program of improvement is justified. We believe that emphasis should be placed on attempting to develop guns of constant base pressure and as nearly constant acceleration as possible through the launch stage. We feel that some improvement can be obtained in light gas gun performance through gas preheating. Our belief in the merits of continued light gas gun development is based on the continued need for experiments of control velocity and shape. We feel that to make progress in the theory we will always have need for a very closely controlled velocity and shape. It is also our feeling that velocities of 40,000 feet per second are possible for light gas guns, perhaps in the near future.

With regard to explosive devices, we believe that these techniques should be exploited to the limit. They are, of course, valuable from many sides. They are valuable first as a means of providing ad hoc data at the forefront of advanced hardware development. Such effects as gross surface effects are very important and at the higher velocities can only be studied by explosive techniques. Of course, explosive techniques provide the only means of getting to the, even close to the meteoroid range and this, in itself, is an extremely important reason for pushing the explosive techniques to the limit. Our understanding of the meteoroid problem is in very poor shape, not only from this viewpoint but from the viewpoint of the environment itself, and we feel that the explosive technique is the best way to continue in this direction. We feel that in connection with explosive techniques that more effort should be expended to obtain accurate information on the mass of the impinging particles. This is one of the shortcomings at the present time and in this regard, we note the ballistic pendulum technique leaves much to be desired, and we urge that a closer look be given to the validity and the improvement of such techniques. This concludes my remarks.

COLONEL STANDIFER

Thank you, Doctor. I have one comment to make here. I feel that there is a considerable amount of work going on, sponsored by everyone from NASA to the Air Force and the Navy in this area that might very well be classified, not because the technique should be classified but because what they are doing is classified. Quite often, very good work is buried under the classification of the end result rather than the techniques. I would suggest that ASD personnel make a complete survey of what is going on in the classified area as a supplement to the information that was presented here in this area. If it's a classified document, so be it, but at least maybe we can take part of the classified work out and separate it from the end product and make it available to the people in general. I will now turn to Dr. Drucker for our other comments.

DR. DRUCKER

I have just a sort of random set of comments. First on the question of checking to see if someone else is doing it. I must say it had disturbed me a little to find out that at this meeting we have only one representative of the Aberdeen group and he's sort of a maverick having been at Brown until very recently. ARPA, as you know, has the hypervelocity program about to start and Aberdeen has been given technical supervision of the hypervelocity program and yet we have no direct representative speaking for this program.

Now, on a more technical point, Dr. Bisplinghoff states that theory and experiments should go hand in hand and of course, I think we all agree; but I then suggest that this requires a physical proximity. There seems to be a great difficulty in this which is a very general kind of difficulty. If you deal with the type of guns which have been spoken of; if you deal with the size of explosives which have been mentioned; this does tend to rule out the ordinary university laboratory as a place in which to do the experiments and that means you have two choices. You take good people out of the universities and you put them next to the guns and the explosives, or you must do something else. Now, if you are going to produce people who will follow up, then you want the scientific and engineering work to be done as far as possible, the base work, at the university; otherwise you will not get students, and I think you should not minimize this particular problem. My suggestion would be, of course, that close collaboration be accomplished in general by much more communication, much more traveling around. We do enough traveling as it is, but I think we could stop off on the way here and there, and we could accomplish collaboration in this particular manner, perhaps.

Now, also, if I may say something about state equations because a number of my fellow mechanics colleagues always have an eyebrow shoot up whenever we mention equations of state, I'm not sure we all speak the same language, but when a fluid mechanics person speaks of the equations of state, or a thermodynamics man does, he very often means precisely that, just equations of state. Equations of state are very nice but in fact they do not exist, in general, for materials we are dealing with. If one just simply assumes that by equations of state one means really constitutive relations in a more general sense, then of course there's no quarrel. I think one must really mean this because, as Dr. Bisplinghoff pointed out, somehow you must match the hydrodynamics theory with the plastic and then with the elastic and this means in a sense there is no equation of state.

Now, the point of scaling laws, I must say I am somewhat less optimistic about this. It's true of course that Reynolds did a lot with a little, but we all know a demonstration of a period and the pendulum. I always wondered, in most cases, if we didn't know the answer we would have gotten it out of what amounts to scaling laws or of dimensional analysis of the problem? And, in particular, if you don't have an equation of state, there cannot be scaling law and this is a great difficulty. I presume what one must do is find a scaling law for parts of the problem. But this is even a more difficult question than has as yet been looked into in any of these hypervelocity problems.

As I say, these are sort of random comments and in no way take away from the list of problems which have been mentioned. I think they simply add to the list and add, in particular, to the complexity of the problems, making a far more complex problem than I think most of us are willing to accept.

DR. GOLDSMITH

I would like to add to the list that Dr. Bisplinghoff has given of research institutes that have done work in the area of equations of state. Particularly, I'm reminded of Stanford Research Institute which has not only done work on equations of state but also in the general area of hydrodynamics of solids and from whence a number of other interesting and important papers have emanated. In addition to that institution, I am familiar with work done at the Franco-German Research Institute in St. Louis, France, which is doing work in the area of what we call hydrodynamic impact. Some fairly original ideas have been developed there which may not have yet traversed the Atlantic Ocean. In addition to this, I would like to make a separate comment on equations of state that has bothered me for quite some time. As used at the present time, these equations are in general a Hugoniot curve with adiabatic or isothermal variations. Only in very rare cases have there been any attempts made to include shear effects in the result of the equation. Now, it seems to me that any tieup with elastic-plastic theories must, of necessity, incorporate some kind of term of this nature, and whether you want to do it on the basis of a shear effect or whether you want to include a viscosity term, is, I think, relatively immaterial. You can tie up only if you realize that there will be a contribution, even at those pressures, and it seems reasonable to me that such a term should be included. This, of course, will complicate the equations enormously but I think the effort of making the attempt in this direction is worthwhile.

Secondly, in the area of hypervelocity impact, I think it would be important to make some assessment of the relative percentages of energy that will go into a shock wave, into shock heating, energy of vaporization, cratering, etc. Some work has been done along this line, but not nearly enough. Finally, I would like to suggest to you the possibility of extending the work on shaped charge analysis so as to include transients both in the velocity gradients and in the compressibility of the jet to arrive at a more realistic answer concerning penetration mechanics, which, of course, is quite similar to the hypervelocity type of impact that we have talked about here.

COLONEL STANDIFER

I'd like to add one comment before we throw this session open to the audience, not to discourage the theoretician but primarily to encourage the experimental man in this field. There are very few equations of state that will compare with $E = mc^2$, and this was not theoretical; it was a philosophical development come full blown. I think it was someone sat down under the apple tree and got hit by an apple and immediately began to worry about the relations of gravity. In this field we are talking about, the usual ideas which occur to you as you are experimenting, for one thing, and as your data begins to develop, as you start to analyze your data and look at it, these things jump out of the page at you one day in relation to scaling laws and things of this nature. So, I'm not saying you should encourage ill-conceived experiments or ill-planned experiments, but I do feel that experimental work can be planned that will be very good and will lead to this type of thing without having to wait necessarily on a beautiful theoretical approach. I think they both should be approached and data should be analyzed more for its total implications and not just necessarily for its exact application for which you designed the experiment. For instance, in steels, until Dr. Bain got millions and millions of tests did he finally come up with hardenability through analysis of his statistical data which gradually developed as a result of these tests. So my suggestion is to correlate and look at this data and put it in one place and then we may see if it is compatible with any of the laws as they come along. I now throw this open to the audience for discussion.

DR. BISPLINGHOFF

Colonel, may I answer a couple of the remarks that were made by the panelist?

COLONEL STANDIFER

Go right ahead, sir. I'm sorry.

DR. BISPLINGHOFF

I do not accept the idea that it is impossible to put a first class hypervelocity impact facility at a university. I think it is possible, and I think it should be done. There has been a tendency to put too many of these first class facilities in the Government laboratories and it has been very hard for the universities who had an interest in graduate school work and in the more theoretical aspects of this subject, to get a first class facility. I'm not saying that the Government laboratories do not have outstanding people to use the facility, but I am saying that there is a tendency to concentrate on ad hoc testing, and I can think of a number of universities with a first class theoretical group who would love to have such a facility and have the means for installing it. With regard to scaling laws, I agree that scaling laws are very difficult. I think that in this problem we are confronted with a problem similar to that of aerodynamics where we went from subsonic to supersonic to hypersonic aerodynamics where the state equations of course changed. However, within a given regime where the state equation holds, the use of scaling laws are very valuable because they permit you to go from one set of data, one set of conditions, to another, and even though scaling laws do pose a very difficult problem, we urge that this be kept in mind as a real objective. That's all I have to say.

COLONEL STANDIFER

Candidates from the floor?

Going, going, gone. Sold to the man over here. Will you identify yourself, sir?

MR. PARKER, CORNING GLASS WORKS

I'd like to inject a comment made to me by an associate who is here with me. I'd still like to know what happens when the target is glass or ceramic material instead of these nice mushy materials like copper and aluminum. I'm making this comment in this hypervelocity discussion, but I'd also wish to direct it to Professor Goldsmith's area on wave propagation. There may well be work done in the field. I'm not familiar with the field. I'm a neophyte here, but I want to throw this out. I want to remind you of some of your glass windshields in such high performance aircraft as the X-15 and the B-70. We will be putting glass windshields into the Dyna-Soar boost glide program and we've had view ports in the Mercury spacecraft for the astronauts to look through, and we will have view ports in the Apollo, Gemini craft and so on. There will also be additional windows, openings for cameras and other instrumental, automatic instrumented observations from different space vehicles. There are glass, there are ceramic microwave windows in missiles, spacecraft, for guidance and communications. In other words, as you look at a space vehicle, the external

appearance is not one of uninterrupted surface of ductile metals; we have a number of openings, some of glass and ceramic materials. In fact, we should note that every silicon solar cell has a cover over it which would either be a piece of glass or piece of synthetic sapphire. All of these things are subject to hypervelocity impact of the type we have been talking about here of micro meteorite and so on, but I'm also interested in the wave phenomena, the flying plate projectile experiments. Then I think that reliability suggests that we should know more about the behavior of these, shall I say, brittle materials at room temperature under ordinary conditions. We should know more about their behavior in the hydrodynamic regime. Thank you.

COLONEL STANDIFER

Thank you, sir. That's my pet subject. I agree with you. The Directorate of Materials and Processes has some work planned. It is inadequate to give us answers in the time we want them, I'm sure. We probably will get it out of some of your hypervelocity impact. One of the things we need to know is the spectrum that we're going to see in space when we get up there. What are the spectra of the meteoroids and meteorites? I think this is definitely directed towards Professor Goldsmith's area also. I want to point out, too, that not only is this not a ductile metal machine that you were talking about, but has ductilities probably in the order of one to three percent, and then you have a coating on the outside of it to protect you from oxidation on re-entry which has, I guess normal tensile elongations of zero. How are the structural responses going to be worked out so that we can build structures that can live with this kind of ductility, one to three percent; or again, in the case of glass, maybe at lower temperatures, zero percent when we define ductility as elongation in its normal tensile test? I would prefer that this part of it be kept until after Dr. Goldsmith gets through. Are there any other areas on hypervelocity impact? Any other suggestions?

BOB BJORK, RAND CORPORATION

I'd like to interject just a couple of comments on scaling laws. First of all, it probably is impossible to scale the whole process of hypervelocity impact. By whole process I mean taking into account also the final stages that involve viscoelastic effects and plasticity. However, the scaling law we did derive, traces the penetration of different projected material into the same target. We have met with a little success that encourages us along these lines. In the hydrodynamic portions you find, I think, that there are three independent parameters necessary to describe the equation of state as compared with one in gas dynamics, namely gas. It takes two to describe the Hugoniot with equal bounds and then you need, in the simplest case, at least one point off the Hugoniot so we have at least three parameters to deal with, even in the simplest case. However, they don't vary over too wide a range which is fortunate, so I don't think it is absolutely impossible to do some scaling. However, we certainly don't know enough about the process to do it now and hopefully, if you solve the hydro portion, you might be taking care of the major fraction of hypervelocity impact. The later states might be just a secondary correction. Secondly, I agree, certainly, with Dr. Goldsmith's comment that somebody should look at shaped change jets analysis. Nobody has done this and this is clearly a hydrodynamic process and nobody has done a successful treatment and yet we know the compressibility effect. I also want to agree with the comment that the energetics should be subjected to some more analysis. You need to know more about these parameters to do it. I have some knowledge of even classified work that is going on and the recommendations were made in light of this knowledge and they still stand.

COLONEL STANDIFER

Thank you, sir. Is there anything else from the audience? In that case I will ask Dr. Goldsmith if he will give his summary after which we'll have a coffee break. Then you can think up some real good comebacks and we'll return and continue the discussion after the break.

DR. GOLDSMITH

I would just like to preface my remarks by saying that some of the comments I wished to make have been pre-empted both by some of the speakers on the platform here and by the audience. This is by virtue of the problem of airplane departure times which required a switch of the presentations arranged for the panel. I don't think the problem is serious except that when I do repeat a recommendation that has already been made, you will understand that I am only underscoring the emphasis which I think it deserves. I will attempt to summarize the subject of wave propagation and structural response in a slightly different manner than either of my predecessors. If I may be permitted, I would like to depart from the podium here and go to the blackboard to outline what I think the general areas are, how they are interrelated and then eventually how the speakers have contributed to one or more of the subject divisions of those topics.

The subject of this symposium is Structural Dynamics Under High Impulse Loading, and I would like to give a brief summary of the present state of this field and perhaps a few words concerning its future outlook. I would like to start off with the first two words, "Structural Dynamics," which imply the response of structures, and here we find that we might divide this into two general areas. The first of these is the initial response, that is, the transient response, which we might class as wave propagation--a loose term, but still sufficiently descriptive of this stage of the phenomenon. The second area would be the resultant vibration, if it exists, or permanent deformation. This I would call the steady or subsequent response of the structure, so that if I may be permitted to label it "vibration" you will understand that if it exceeds the elastic limit it will not be a vibration in the classical sense. I will put that down as Class 2. Now, to study these, we have at our disposal in the first instance the equation of motion and some set of constitutive equations, say, the equations of elasticity relating stress and strain. These are derived on the basis of experience; they have their foundation in a much more esoteric formulation of the general behavior of materials, which was discussed earlier in the session. However, the complexity of the equations of motion is governed, to a large extent, not only by the shape of the object under investigation, but also by the time of impact. As a result of this, we find ourselves with one-, two-, and three-dimensional propagation problems. The majority of the discussions in this room and elsewhere in the country have been concerned with one-dimensional problems for the very good reason that they are by far the simplest to treat. Two- and three-dimensional analyses based on equations of motion are extremely rare. In fact, the only three-dimensional--strictly speaking--asymmetric problem, is the classical problem of the circular rod executed by Pochhammer and Chree. Since this analysis which was performed in 1882 and 1888, a tremendous effort has been exerted, particularly in the last 20 years, to devise simpler mathematical relations which would approximate the behavior of the circular rod on a one-dimensional basis. At least 200 publications have been issued in this country and abroad on this particular subject, each with its own particular variation of a correction which would improve the wave velocity spectrum of these one-dimensional approximations relative to the Pochhammer equations which are considered to be exact. When I say "exact," I put this in quotation marks because those relations were

developed only for an infinite cylinder and for a steady-state sinusoidal input. Obviously, some investigation should have been performed, which has not been undertaken to date, as to what would happen if we have a transient; but if we believe Mr. Fourier, it is to be expected that we can solve those particular problems without too much difficulty, at least, in principle.

The vibration of the type described by Professor Hoppmann has in classical times been formulated according to the standard normal mode pattern. It is obvious that continued extensions in this direction are badly needed, particularly in the area which he so well described, where we no longer have one-dimensional propagation with an isotropic homogeneous structure, but rather orthotropic bodies, and in the long view, for heterogeneous materials such as we might find in the area of rock dynamics, which has taken on an increasing importance today. The equations of motion are, of course, very much more complex when we go beyond the one-dimensional case. For two- and three-dimensional processes, most of the problems have not even been formulated. Even if formulated, no attempt has been made at any numerical solution. Therefore, I must underscore Professor Symond's comment about the exploration of additional numerical solutions, even in the elastic regime, and very much more so in the plastic region. I believe he will agree with me when I say that one-dimensional plastic wave propagation has been explored to a considerable extent, even for the idealized case of rigid perfectly plastic solid, and to some more limited extent for the elastic. However, no two-dimensional approaches to plastic wave propagation in plastic solids in the classical sense have been formulated to the best of my knowledge.

Along these lines, I would like to very briefly indicate where the talks that we have heard in the session on wave propagation and structural response fit. Both Dr. Percy and Dr. Filbey, as well as Mr. Arenz, have talked about uni-dimensional propagation. In the first case, we were dealing with the case of constant strain in which we took a large plate, excited it at the distal end in some manner, measured the disturbance at the front face--presumably at the center--so that the effects of rarefactions from the sides would not significantly distort the pattern from that of a propagation process. The obvious disadvantages of this procedure have already been cited; the fact that it is impossible to make measurements in the interior of the specimen, and the fact that there may be reflections or rarefactions from the side. Also, to some extent on the basis of personal experience, there seems to be a bit of unwieldiness in the construction of the equipment as opposed to that of the second speaker, Dr. Filbey, who talked about constant stress states in which we were concerned with the analysis of propagation in a rod. Again, the disadvantage of this technique has also been mentioned; specifically, the fact that this is not really a one-dimensional phenomenon, due to radial inertia, that even if the load is applied uniformly, you have an axisymmetric problem which will increase the level of difficulty considerably. Mr. Arenz considered the next step above a perfectly elastic substance by incorporating the possible effects of viscoelasticity and examining the model on a simple linear viscoelastic basis in the hope that this simple representation would give a reasonably good engineering approximation of the actual behavior of the material. He was aided by the very powerful photoelastic technique, which he says ought to be called "photoviscoelasticity" to emphasize the fact that the materials concerned are indeed viscoelastic. By means of this analysis, which is not by itself a powerful tool, but rather a good application of a simple idea, he was able to correlate theoretical predictions with experimental data. I might add, from personal experience, that in examining photoelastic propagation, I have found that for many materials, particularly the hard plastics, a good representation of the phenomenon can be obtained even by purely elastic considerations. However, when you work with the more viscous materials, particularly the low modulus materials, it seems obvious that a

viscosity term or more than one viscosity term must be added to adequately predict the general effects.

Finally, Dr. Hoppmann gave an extremely able exposition of not only the kind of work that has been done in the past, but also of the future prospects for structural response in high impulsive loading, and this brings me to the last point of the summary, and that is the question of what do we mean by loading? As far as the applied mathematician is concerned, he will take the equations of motion with a given input function, crank it through the machine or, if in rare cases it is possible to get a closed-form solution, he can do it at his desk and obtain an answer. But we have really no idea what a structure might be subjected to in terms of either nuclear blast, meteoritic impact, etc. I would thus recommend that the nature of the loading function with respect to physical reality be examined a bit more closely.

With respect to recommendations for future research, the speakers in the session made the following suggestion which I would like to second, namely, that in the elastic-plastic regime, large deformation theory be incorporated in conjunction with a thermodynamic approach to the phenomenon. This, of course, again increases the level of analytical difficulties considerably, but I believe it is something that is absolutely necessary in order for us to push the analysis above the level of a few percent beyond the elastic limit as far as strength and deformations are concerned. Secondly, I would like to support Dr. Bisplinghoff's proposal to attempt to join the elastic-plastic theory to the hydrodynamic theory. Please note that I am approaching this subject in a different manner from his viewpoint, but I think this junction is absolutely necessary. For example, in the case of an underground nuclear explosion, everything in its immediate vicinity is vaporized. Just beyond this regime, a surrounding solid will be in a liquid state, but sooner or later elastic-plastic waves will be propagated, and beyond their limit the elastic phenomena will still be in evidence. All of this is, of course, a continuous physical process and should be capable of description in some way.

Usable constitutive equations are badly needed by the applied mechanician. By this I mean that we would lean on people like Dr. Ericksen and the metallurgical personnel of the very high caliber of Dr. Dorn and his associates to give us equations of state that we can use for the prediction of phenomena by incorporation in the equations of motion. However, I am not yet prepared to state that we ought to include the third and higher order terms in the acceleration side in the next 10 or 15 years. It may turn out that this might be desirable at some much later time. I would recommend the study of waves in anisotropic nonhomogeneous elastic materials. We have done very little in this area because we would like to concentrate on the simpler problems first and leave the more difficult ones to future generations.

I would finally recommend in this general area of structures that we examine the practicality of the dissipation of energy by the propagation of waves not only along bars and plates or other structures of simple shape, but around connections such as pins and welded joints. Very little, if anything, has been done in this regime and, of course, it is of extreme importance to the military as to whether their systems will be able to survive certain hostile environments. Along the experimental line, I think we should make an additional effort in the area of both photoelasticity and photoplasticity which, even though at the moment appearing to be limited to one-dimensional or two-dimensional phenomena, might, with considerable ingenuity, be extended to three-dimensional processes. Some ideas of this kind are germinating up here (pointing to his head), but are not yet capable of expression; however, I think we should examine probabilistic impact in terms of wave propagation.

For example, if we send a vehicle through the atmosphere, we cannot predict whether a meteorite will hit it at a given point, in a given way, or at a given velocity. I think we should very definitely examine this problem on a probabilistic basis, at least with respect to an input. Some beginnings in this area have been made, but his subject is still a long way from the area of competence which other topics in this general domain have achieved. I would also like to reply to the gentleman from Corning Glass Works with respect to what has been done on glass. I know of a number of experiments in which pellets have been fired into glass for the purpose of studying fracture and crack propagation. Professor Schardin at the St. Louis Institute has conducted experiments during the last 30 years the results of which have been published in many different journals in this country, but of course this has not been done at hypervelocities. Perhaps it would be simple for one of the gentlemen who has such a facility to substitute glass for one of the ceramics, or plaster of Paris for the steel, aluminum or copper that has been used so frequently in the past.

Finally, I would like to mention some of the people who have done work in the area of elastic-plastic wave propagation. In the United States, Professor Miklowitz at Cal Tech published a very excellent summary approximately two years ago in Applied Mechanics Reviews covering work in elastic wave propagation in rods, beams, plates and shells. Professor Eringen, Purdue University, has been actively engaged in elastic wave propagation during the same period. In the experimental area, Professor Ripperger of the University of Texas, in conjunction with Dr. Abramson of the Southwest Research Institute, has done very excellent work in experimental elastic wave propagation. Others, for example, Professor Curtis of Lehigh, have done work on second-order effects in elastic wave propagation, all of which ties in with the approximations of the Pochhammer theory that I indicated here earlier. In the area of plastic wave propagation, the only name that comes to mind is not a person, but rather an institution, Brown University, where Professors Prager, Drucker, Symonds, Lee, and many others have worked very actively and successfully in the prediction of the propagation phenomena in the elastic and the plastic regimes. This concludes my summary on this subject.

COLONEL STANDIFER

Thank you, Doctor. Representing my friends, the other pilots in the Air Force, I highly encourage first priority on the design of dependable joints and pins. This gets pretty close to home whenever they don't work. At this time I will give our other panel members a chance to make any comments they choose, particularly as it affects other areas.

DR. BISPLINGHOFF

I pass to the floor, I thought it was an excellent presentation of the subject.

DR. PLASS

I would like to say some more about the thought that Dr. Goldsmith mentioned, namely, the need to examine or develop techniques for description of structural behavior when more than one dimension is involved. I think that a fruitful source of techniques would be the various variational principles which have been developed by mathematicians over the years. Through the use of variational principles and judicious selection of meaningful variables in the particular physical problem, either vibration problems or wave propagation

problems, theories may be developed to approximate the behavior of any two- or three-dimensional structures, or built-up type structures or connections of various types.

Another comment that I would like to make has to do with something that has been concerning me as an educator. Many of these things that are mentioned at meetings of this sort, and at technical meetings of various societies, are published in journals and sometimes just in reports and are placed on library shelves and places that generally are hard to find; and sometimes it is difficult to know where these papers are and what is in these papers. Attending meetings of this sort, of course, makes you aware of the existence of certain types of work, where you can go to look it up and what you can do to add to this work. The thing that really concerns me is the apparent inversion of effort that seems to be going on now between the universities and industry. In some instances, industries are doing research work that is far out ahead of anything that anybody in the universities is doing. If we conceive of the body of scientific knowledge, particularly engineering knowledge, as a sphere and the efforts made by various research men to expand the boundary of this sphere as being spikes sticking out of this sphere, we find that in many instances industries have bigger spikes than universities. Quite frequently you find that because the emphasis placed on the development of these spikes by agencies such as the Air Force, Atomic Energy Commission, Navy, or other groups who are trying to accomplish certain tasks, such as NASA trying to launch vehicles to the moon, great sums of money are spent to develop techniques, to build certain types of instruments or certain types of vehicles. Most of these funds I imagine are spent in industry. As a result of course they build bigger spikes.

What should the role of the university be in this sense? Should we just try to imitate this and follow along in the footsteps of industry, or should we try to build spikes of our own, or should we ignore the fact that we are in a race to make spikes and try to fill in the sphere at the bottom a little bit. There is a lot of knowledge represented by many papers that are published here and there that needs to be brought together. This is particularly true in the educational institutions because the role that the Professor in an educational institution must fulfill is to bring this knowledge to the students. It seems like in many instances this is forgotten.

To make this knowledge available to the student, we have to sort over much of this material and throw away the things that don't look too promising and save things that do and assemble them in a form that the next user can benefit by and perhaps that industry can also benefit by. I am thinking of work such as that done by Lord Rayleigh or Timoshenko. Timoshenko, for instance, took a lot of people's research papers and was able to assemble their essential features into a variety of very useful books, books which find great use today in the university classrooms as well as in research laboratories and universities and industry. I think that work of this sort, although it isn't very glamorous, needs to be done and I think the university is probably the place where it can be done with more ease and perhaps more thoroughness because the pressure of deadlines is not upon the university as it is upon industry.

COLONEL STANDIFER

I probably was negligent in not pointing out from the word go that this conference or symposium is sponsored by the Directorate of Aeromechanics of the Aeronautical Systems Division and the Office of Aerospace Research. So any recommendations that come out of this program will go to both organizations and will be used jointly by them as well as other

divisions and directorates of ASD. So don't feel that things you are proposing must be applied only in consonance with ASD's applied research or development requirements. The Office of Aerospace Research is a joint sponsor and is interested in the very type of things that some of you university professors particularly are interested in doing. I probably was negligent in not pointing this out although I guess I just assumed that this was in the original publicity for the symposium.

May we have discussion from the floor? I'm going to ask Dr. Goldsmith if he will run this particular session.

DR. GOLDSMITH

Dr. Hoppmann?

DR. HOPPMANN

I don't like to pre-empt too much time of other people, but there are a couple of remarks that I would like to make.

COLONEL STANDIFER

They told me, for the other speakers as well, that unless you talk into the microphone we miss some of your comments. Hold it up towards you and identify yourself for the record so we can tell who is talking.

DR. HOPPMANN

I am Dr. Hoppmann from Rensselaer Polytechnic Institute and I would like to comment on a few remarks made by Dr. Plass and Dr. Bisplinghoff. I might begin by saying that I am certainly highly edified by what has gone on here with regard to the platform. I think very serious statements have been made which deserve very serious thought.

Now, Dr. Plass mentioned the university and I would like to say that we feel that in the university, or technical school, we are relieved or protected from what I might call the problem of immediacy. The military is afflicted with it and industry is afflicted with it to an extent. They have to produce for the here and now. You will notice that always in time of war the universities close down. Why? Because everybody rolls up their sleeves and goes to war. That is not accidental. I wish people would bear that in mind. I would like to say that the university or technical school has two problems; probably the first of which is turning out good students. Certainly this is a product that we are probably pretty short of. Everybody agrees that there are good students; that's not accidental. The other thing is the research that is done in the university or technical school should be free from the immediacy of the military or other demand. Now the concrete point that Dr. Bisplinghoff made, among many good things, was that it might be a good thing to have this large gun on the campus. Now I can't say that; I'm not sure just what this would amount to, but it is a good case in point and he mentioned that some universities would profit by this. How he knows this I'm not sure, but he certainly has some basis for making the statement. But I would warn people to notice this: If you put a big installation on a university campus, the

university is bound ipso facto because the President must now tend to support the installation. That means somehow he must constrain the people to look towards it almost as a great god or pagoda to worship. You cannot get around it. You must be very careful when you make this kind of a suggestion.

I would like to take this opportunity to introduce a plea for literature. We all make a plea for this in general, in the same fashion that we speak of the clouds and the sunshine and everything else; but I mean something specific. We can get junk, we can get things that are not junk, and so on and so on. The Office of Naval Research has published some excellent literature. Jack Riley, who is here, has made some of their publications available to me. There are two that I would like to mention; one is on elasticity and one on plasticity. I mentioned these publications in my paper and I think they are excellent; and they are excellent because the people they got to write them apparently put some sweat, blood, and labor into the production of them. They represent real thoughts, like Mindlin's paper on elastic waves. The elasticity volume is a notable contribution to this field. Certainly it is limited, but it represents a real contribution. I would finally like to wind up by saying that I tried to make it clear in my paper that I was not trying to indicate that what I was talking about was a complete state-of-the-art. There are many other references that can be made. Dr. Goldsmith brought some out, too, and I don't think that he more than touched the surface. There are many other sources, and we should use and give credit to them.

I would like to emphasize this job that the Office of Naval Research has done. It is a paragon in the field and may be emulated by others. Thank you.

DR. GOLDSMITH

Thank you, Professor Hoppmann.

DR. EUBANKS

Dr. Goldsmith almost made an error which affects what I am going to say. I am not Professor Eubanks, I am Doc Eubanks of Armour Research Foundation and the difference is important in connection with my remarks. I feel that many of the comments which have been made here, or many of the papers which have been presented, are of such quality that the entire conference has been on a very high academic level. I have the feeling that many people are not aware of the abysmal ignorance of those of us who have to solve these problems or work them. As an example, Professor Goldsmith mentioned that we don't know the inputs. We know very little about the inputs and very little about their effects. We cannot for example, compare very well the effects of a change in rise time to the effect of a change in amplitude, nor the effect of a change in pulse duration. These items we do not know when we are looking at the input, and thinking of this problem of structural response, and trying to analyze it. You see, the things I am talking about are perhaps immediate, but I believe that they are also of overall importance from the point of view of our understanding of the problem. We could compare this with the straight forward analysis of the boundary value problems in plasticity, versus applications of limited analysis problems brought up by Professor Drucker. We do not have a limit analysis type approach for shock wave studies. Perhaps the shock spectrum approach is as close as we have. Yet, if you talk to anyone who has been working in the field, who has been using the shock spectrum, he will tell you this is something I'm using because I have nothing better; and he himself

realizes the logical and physical fallacies associated with it. So, that my plea is for an approach to the structural response problem which is based, perhaps on more gross phenomena, but which will permit us not only to analyze better but to set up tests which are more indicative of what is happening in the field and to experiment on equipment and actually determine whether or not this equipment will survive; whether or not we do have a decent damage criteria. We do not have one at present.

DR. GOLDSMITH

Thank you, Dr. Eubanks. I think the problem you have touched upon is one which has already been alluded to here, namely, the question of whether we need immediate criteria on damage, so that it can be used tomorrow in a service installation, or whether we should look ahead, ten, fifteen, twenty years, to see what the problems are that are to come and prepare for them adequately by basic research now. I don't believe the two views are irreconcilable; but they may be irreconcilable for a single individual or institution. I think there is a place for both.

DR. JONES

Orville Jones, from Sandia Corporation. I would like to kind of take up the glove on experimental measurements that was asked for in Dr. Drucker's conception and use that as a pretext for an area which I think should be mentioned here and is going by. We have high pressures available to us here. One of the things we have been talking about is mechanical behavior almost exclusively. Electrical behavior under high pressure is also very interesting. I mention this in the context of mechanical measurements in that we undertook to study quartz for its electrical and optical properties as well as its mechanical properties. Out of these studies of quartz came a useful apparatus on the electrical side; also out of it came the quartz gage which was observed just as an incidental to studying the electrical properties. It looked like it had a profile that was coming through these specimens. This has been a very nice measurement technique since there is no ringing or anything at all. With most mechanical measurements you can gage length, or if you have some finite inertia you have to worry about ringing eventually. Out of these quartz studies has come this technique which is an interesting variation. Other studies which are quite interesting, and that we are quite excited about pursuing, are the electrical effects in semiconductors under high pressures. This is being undertaken on both a static and dynamic basis, and I think this area is one which is very very interesting from the dynamic wave propagation standpoint. Also, through some of these very unusual effects that occur, it may give new measuring devices. The state-of-the-art of solid state physics is changing so rapidly that on the one hand it would be nice to refine our mechanical measurements as such; on the other hand, we should try and keep pace with some of the exciting new developments that may come along these lines too. That is all I have to say.

DR. GOLDSMITH

Thank you, Dr. Jones. If I may just add a word on this subject; in my summary I alluded to the subject of photoelasticity. We have in this room two very distinguished gentlemen who have worked a great deal in this area, Professor Durelli and Professor Duffy. The latter left; had to catch a plane. In conjunction with your remarks concerning solid state physics and additional properties other than mechanical, it was very interesting to me to

go through the literature and find that the photoelastic effect, which is one of the solid-state physics effects, was discovered in 1816. It was quantitatively formulated at that time, although formulated incorrectly, and even today the effect has not been completely defined. When I asked some of the foremost solid-state physicists in the world whether anyone was working in this field, in the area of solid-state physics, I obtained a negative reply. This obviously bodes ill for a correlation of mechanical properties with either electrical or photoelastic or ferroelastic effects unless some real effort is put in to gain a basic understanding of these phenomena. Now, supposedly, this sort of thing is in the area of physics and not in the area of engineering; perhaps engineering will have to take it over by simple default and I would just like to add this remark to yours, Dr. Jones. The associated problems we have to worry about are better instrumentation for a substantiation of our theories and for simply observing phenomena in an interesting and useful way. Are there any other comments?

DR. JACK RILEY, OFFICE OF NAVAL RESEARCH

I would like to thank Professor Hoppmann for his kind remarks. To avoid a little rush of inquiry, the reports that he referred to are the proceedings of the first and second symposia on Naval Structural Mechanics which were published through Pergamon Press. Those symposia were not unlike this one sponsored by the Air Force. Thank you.

DR. GOLDSMITH

Are there any other suggestions from the floor? We would like to have some specific recommendations as to what some of the members of the audience think would be a fruitful direction for future investigations in the field of wave propagation and allied areas that we have discussed or will be discussing.

KIRSCHNER, CORNELL AERONAUTICAL LABORATORIES

My name is Kirschner, Cornell Aeronautical Laboratories. You mentioned the importance of analyses involving inhomogeneous and isotropic materials. I would like to make one comment in connection with this, and that is, that in some cases it may be desirable to vary the anisotropy of the materials. I would like to point out anisotropy exists on two levels; one is the anisotropy of the crystals and the other anisotropy of the pieces of polycrystalline materials. The anisotropy of crystals has been varied in some cases. Then in connection with the remarks of the gentleman here from Sandia, there is one other item to add and that is that there is a Russian paper on electrical properties of sodium chloride under shockwave loading.

DR. GOLDSMITH

Thank you very much. There is always a problem caused by lack of communications in being able to read all the publications in the world at the same time. Perhaps one of the Government agencies might consider the possibility of better dissemination of technical works published in areas other than the United States for distribution to those interested. Are there any other comments? Dr. Durelli?

DR. DURELLI

By way of suggestion, I believe there is a technique which may be an approach to a solution of the problems in the field of wave propagation. I think it probably will contribute to a better understanding of some of the most complicated phenomena we have alluded to today. I would like to suggest two simple problems which have not been solved completely. One is this simple problem of structure. It happens now as dynamic problems analogous to what I think happens to the tensile specimen. This is indeed a problem for whoever doesn't understand it well, but whoever goes to the lab and likes to have a really uniaxial perfect tensile specimen, this is extremely complicated. Now in dynamics, this problem is at the end and we have approximated the uniaxial strain or uniaxial state of strain we saw yesterday. Well, if you do some photoelastic work on this, you will see how complicated it is. It is not what you expect. Up to the present time, there is no such thing really as a solution to the three-dimensional problem of the changing proportion of space and time. Now the techniques for solving this problem have been developed up to a point where its solution can be approached with the hope of solving it completely. I think that the solution of this simple problem would allow a much better understanding of what we are doing with our rods, square plates, and square bars. By hitting a specimen at one end, we see the waves traveling back and forth; we hope to break one end of the brittle specimen and in that way determine the length of the wave, and so forth. All this is based on a knowledge of the wave propagation which is still not complete; knowledge I think could be completed with this technique now being developed.

Another problem is that of impact of semi-infinite state. You drop a weight and produce an explosion in semi-infinite space, and generate wave propagation over a medium. Now this is treated as a three-dimensional problem. I think these techniques are to a point where with a bit further effort, this problem can be solved in time. I would like to suggest these two basic problems.

DR. GOLDSMITH

Thank you, Dr. Durelli.

DR. BISPLINGHOFF

Before leaving this subject I would like to reply briefly to Professor Hoppmann's remarks that my suggestion may result in the hanging of a millstone around the college president's neck. I think the university role is one of education. I think this is primarily the role that the university plays and as an educational entity the graduate of the schools must choose research that fits the needs of graduate education. Now the interaction of mechanics and metal physics and structures, represents, I think, one of the most interesting interdisciplinary goals which we have at this particular time.

Guns capable of producing hypervelocity impact are one of the cheapest ways to stimulate students to thinking about these disciplines and bringing them together. A gun is very inexpensive to maintain; it is fairly expensive to buy. It is nothing like a wind tunnel. A wind tunnel can be very expensive to buy and very expensive to maintain, and I think a light gas gun falls in a different category. There are, of course, many ways of stimulating a student to think about a combination of interaction of these disciplines; metal cutting research is one possibility; energy conversion is another. But I believe that the kind of thinking a light gas gun would stimulate, understanding the mechanisms of impact, cratering, and wave

propagation through the material, is one of the most inexpensive and interesting tools that one can bring into a graduate laboratory at this time.

DR. HOPPMANN

I would like to take this occasion again to insist on the sovereignty of the professor. Dr. Bisplinghoff was, I believe, a professor. Isn't this correct? Now we do not defer to anybody outside whatsoever, industry, Government, or anybody else. As soon as you do then you are being operated by a backseat driver.

COLONEL STANDIFER

I would like to make a comment. This is the only facility recommendation that has come along. I would like to leave one thought with you as university professors, research institute representatives in particular, as to the value to you of in-house Government research, and facilities to do exploratory work. Generally speaking, it is much easier for me as Director of Materials and Processes, for instance, to fund a facility at your institution than it is to get it appropriated through Mr. Cannon and some other people in Congress, not counting the men on the Appropriations Committee. So facilities as a general thing are extremely difficult to obtain for in-house Government laboratories; much more difficult than they are to fund at universities. I feel that facilities should be at both places, and further that a facility is nothing but a tool. It is used just like any other piece of equipment you are going to need to stimulate your students. If it becomes a god to the student and he becomes a slave to it, then you as a professor should not go and prostitute your Ph.D. student to a program where he is going to measure the velocity of little bullets going down a tube unless no one has ever measured it before and he is coming up with something that is completely unique.

Now then, concerning research in a Government laboratory, I want at least 35 percent of my technical manpower doing in-house research because I feel that the quickest way to become a contract officer, procurement-type contract man, is to get away from the laboratory and not get your hands wet. Where are the ideas for solicitation for proposals coming from (other than having groups together like this) if you don't have people in the laboratory doing work? How can they communicate with the university professor? Do you as a professor or project engineer in a research institute want to bring your proposal to the procurement officer, or do you want to bring it to a colleague in the Government military or civil service? If you don't have these facilities in house and don't do a given amount of in-house research, you will then have a person who can only quote you the regulations, take the SAB Reports (although they are very good) and say "Does this fit into the SAB Report? If it does check it off and fund it." Do you want to bring it to a colleague, or do you want to bring it to a procurement officer? Now this is about what in-house research means to a laboratory at Wright Field and to the OAR.

Now I wanted to leave that thought with you. I don't think that there is any conflict concerning the gun that I am financing for some work here at the laboratory. In the first place, can you make one of these guns and make it do what we want it to? The main reason we really want it is to find out what happens to some of our coatings and things along this road and to get enough feel to find out if we can contract for the work to be done. If you can't define your work, it is not very good to go out and contract for it.

MR. JOHN CURTIS, GENERAL MOTORS

The subject of ballistic ranges has been brought up several times in the discussions and I finally feel compelled to say a few words about it. A complete ballistic range suitable for accelerating a projectile up to the velocity of ten thousand feet per second can be constructed in the area the size of the speaker's table for the cost of a few thousand dollars. A complete ballistic range capable of velocities up to thirty thousand feet per second can be constructed in an area the size of the speaker's platform for a cost of a few tens of thousands of dollars and this to me is not a millstone around anybody's neck. The areas open for research by one of these devices are tremendous. The disciplines involved are very diverse. Everything from fluid mechanics, to materials, to electronics, to structural dynamics. I think a ballistic range is extremely suitable for university research.

COLONEL STANDIFER

Could I suggest again that it depends on the program that the professor undertakes as to whether or not he wants or needs one of these pieces of equipment. At the end of the program if he doesn't want to carry on any more, it is junked or given to someone who wants it for something else. To me, unless you are talking about something the size of the Arnold Engineering Development Center, which I don't think any university is really talking about, it is very seldom that the tool requires the program. I don't think that you should ever have a program just because you have a tool. This is what you encounter in Government laboratories. They fight for years for a piece of equipment and finally they get it. Unfortunately, it or the program is obsolete. If the program is continuing, it is always behind time because they have this piece of equipment they must use; this is basically a mistake. So I think it is a mistake any time you regard a piece of equipment as anything other than a tool to do the work. This is why I think the facilities and equipment have to come concurrently with the program, and this is one of the reasons for including the facilities subject in the remarks section here today. I think we have had a good healthy interchange.

DR. GOLDSMITH

I would like to close the session by making two remarks. In conjunction with this slight controversy that has been going on here, I would like to state that for years I have been trying to get my parent institution, one of the wealthier institutions of higher education, at least by normal standards, to buy me a high-speed camera. They have very successfully resisted this for a period of six or seven years, at a time when I could very well have used such a device.

I finally persuaded one of the Government laboratories to let me have one of their surplus cameras, which is about 20 years old, but still approaches what I would like it to do. If I had been able to get the appropriate funding from a Government agency at the time it was a modern piece of equipment, I am reasonably sure I would be a little bit closer to what some of the Russians have published than where I am now. I do not at any time wish to minimize the real importance of Government subsidy for a piece of equipment that is normally out of reach for the average college professor when that piece of equipment is demanded by the research activity of the man.

Finally, I would like to say that I think Dr. Hoppmann has ably expressed the role of the university but I do believe he has left out one important point and that is philosophy. I feel

that a university definitely exports philosophy in terms of its people, in terms of its attitudes, and I would like to say that it has been a sort of surprise to me to see the session on wave propagation riding so high on the foundation of philosophy.

COLONEL STANDIFER

Since Dr. Bisplinghoff must leave and there may be others who will have to leave early, I want to interject some of my final remarks here. I want to thank the Session Chairmen for doing such an outstanding job of coming up with excellent summaries. They were, in a way, flying by the seat of their pants, and still came up with a real feel for the program and fine recommendations for research objectives. In behalf of ASD and OAR I want to officially thank them very much for their efforts, and to thank all the speakers and each and every individual that attended and contributed to our program. At this time I will turn the mike over to Dr. Plass who will give us his summary on the Fracture Phenomena Session.

DR. PLASS

I am very glad that Colonel Standifer issued his words of thanks before he heard my comments. When I was invited to be the Chairman of the Session on Fracture Phenomena, the letter of invitation said that I was an expert on this subject which, of course, is not true; but since it said so in the letter, I'll accept it. I have done no work in this field; I have only listened to papers and talked to people involved in problems of this sort and had seminars with graduate students and so on. Because I am not an expert, I think that my summary had better be rather brief and then I will call on the authors who participated in this session to see if they want to add any comments of a summary nature.

I looked over the material that was in their papers and tried to think about how it was related to other material which had to do with fracture phenomena. It seems that the investigation of the fracture problem is in a rather advanced state of development, compared to its status of 10 or 15 years ago. Originally the fracture problem was oversimplified in that it was thought only the state of stress had to do with whether or not the material would fracture. For instance, in courses in strength of materials we would display a stress-strain curve to our students and say that fracture occurs when the stress reaches this particular level. Later on when the problem was refined to some extent so that two- or three-dimensional problems could be discussed, we taught the students that principal stresses when inserted in a particular formula gave a certain numerical value, then plastic flow would occur or breakage of the material would occur if it happened to be brittle. I have learned by listening to papers such as those that were given this morning that this situation is not as simple as that. The fracture problem had to grow up a little bit and I'll say the investigation of the fracture problem had to grow up a little bit and had to concern itself with the processes of fracture.

By processes I mean whether it is a ductile or brittle type of behavior or somewhere in between. The fracture problem had to be concerned with various parameters which affect the fracture such as the time during which the load is applied or the temperature of the specimen which is being subjected to load or whether the stress field is uniform or nonuniform. Much work has been done with the propagation of cracks in the material. The paper we heard this morning had a rather extensive bibliography of investigations which were concerned with the growth of cracks, rate of the growth of cracks, and so on. In

addition to this particular problem other parameters need to be included such as material property parameters, whether the material is isotropic or nonisotropic; whether it is homogeneous or inhomogeneous in character. In addition to the very act of describing the fracture process and the parameters affecting it, various environmental studies also are involved. By environmental effects I mean, whether or not the load is steady; if it is gradually or suddenly applied; whether or not there are wave problems to be considered in the specimen or if a steady field of stress would be sufficient to describe what is going on in the specimen. In addition, fatigue is a very closely related problem. The fatigue problem is certainly an important factor that must be considered in aircraft and space vehicle design, especially in those cases where the same piece of equipment is to be used more than once. Even if it is to be used only once, the fatigue problem is important if during that use the equipment will be subjected to a type of load which varies with time between two limiting values, through many cycles. In the fatigue problem, as well as in the crack propagation problem, the small cracks in the material which originate by the fortunate or unfortunate, as the case might be, migration of dislocations coalesce at one location. The investigator must come up with some kind of a theory which explains how these very tiny cracks become magnified into larger cracks and eventually into separations of pieces of material. We need to know how this happens so that we can avoid it, if possible, by appropriate design in structural members.

This morning we heard three presentations on this general subject of fracture phenomena. Dr. Irwin's concerned itself with the problem of crack propagation. Dr. Hsiao talked about time dependent effects. Finally, Mr. Lundergan's paper had to do with a particular type of fracture phenomenon which occurs under rather complicated stress conditions where the waves traveling in the material create a region of high tensile stress for a brief time and during this time the material suffers separation or fracture. In fact, at certain times, enough of the material is so affected that some rather large pieces can fly off.

I think I'd better stop talking about the fracture problem. As I said, I am not the most qualified man in the world to summarize these papers. Would someone in the audience, or perhaps one of the authors, like to say a little bit more about this and perhaps summarize it a little more effectively?

DR. IRWIN, NAVAL RESEARCH LABORATORY

I had a comment in mind during some of the discussion on wave propagation which I feel applies to this field of fracture studies. It has to do also with facilities. It seems to me that probably in thinking about normal loading tools for doing work in a mechanical testing laboratory and then skipping way over to loading tools for loading in microseconds, we may have passed over a range of loading tools which are of value and yet frequently are not available. We really need, then, mechanical testing of the whole spectrum of loading speeds, from the static to microseconds. If you will look carefully at what is available in the usual testing laboratory, you will see that the dynamic loading tools are in short supply, not just in the microsecond range, but all the way through. This is important to fracture studies because you apply fracture studies to strain-rate sensitive materials; in studies at static speed, we do not usually get the reflection of brittleness we frequently see in actual structures. We have to apply the loads dynamically. This doesn't mean necessarily microseconds, but we may have to load over a range of loading speeds to measure the crack-toughness of the property in a realistic way. I believe more thought should be given to just what are the proper loading tools for dynamic loading in a wide range. Probably some part of an Air Force program could be generated for support of these facilities.

COLONEL STANDIFER

Are there any other remarks from the authors themselves? If not, how about from the floor? (No reply.) Then to summarize the symposium, I think we have looked at it from the fundamental through its application to structures. Some of you might have preferred it to have been more fundamental, others more in the area of design and applied mechanics to design. However, I am certain that both OAR and ASD have benefited immeasurably from this symposium. I hope all of you, likewise, have benefited from the interchange of ideas as well as from the presentations and lively discussions. Thank you. The symposium is adjourned.

APPENDIX

SYMPOSIUM PROGRAM

SYMPOSIUM ON

STRUCTURAL DYNAMICS UNDER HIGH

IMPULSE LOADING

17-18 SEPTEMBER 1962
DAYTON-BILTMORE HOTEL

CO-SPONSORED

by

AERONAUTICAL SYSTEMS DIVISION

and

OFFICE OF AEROSPACE RESEARCH

PROGRAM

Symposium Chairman

Major General R O Ruegg

Symposium Deputy Chairman

Colonel L R Standifer

Sunday, 16 September 1962

1700-2100 Registration - Main Lobby
Dayton Biltmore Hotel

Monday, 17 September 1962

0800-0900 Late Registration - Information
Booth outside Junior Ballroom

0900 Opening and Welcoming Address
Major General R O Ruegg
Commander, ASD

Keynote Address
Major General M C Demler
Commander, R and T Division

0910 TECHNICAL SESSION I

MECHANICAL PROPERTIES OF SOLIDS

Introduction - Session Chairman
Dr. Dan Drucker
Chairman Physical Sciences Council
Brown University

Dynamic Properties of Matter under High Stress
Thermodynamic Descriptions
Dr. Harold L Brode
RAND Corporation

Oriented Solids
Dr. J L Ericksen
John Hopkins University

Defects in Solids at High Velocities
Dr. Allen N Stroh
Massachusetts Institute of Technology

Dynamic Analysis in Viscoelastic Media
Dr. M Williams
Mr. R J Arenz
California Institute of Technology

1200-1330 LUNCH

1330 TECHNICAL SESSION II

WAVE PROPAGATION PHENOMENA
AND STRUCTURAL RESPONSE

Introduction - Session Chairman
Professor Werner Goldsmith
University of California

Wave Propagation in Uniaxial Strain
Dr. John Percy
Massachusetts Institute of Technology

Uniaxial Stress Conditions
Dr. Gordon Filbey
University of Pennsylvania

COFFEE BREAK

Dislocation Concepts of Strain Rate Effects
Dr. John E Dorn
Professor Frank Hauser
University of California

Some Problems in Dynamic Response of Two Dimensional Structures
Dr. W H Hoppmann, II
Rensselaer Polytechnic Institute

Monday, 17 September 1962

1900 TECHNICAL SESSION III

HYPERVELOCITY IMPACT

Introduction - Session Chairman
Dr. Raymond Bisplinghoff
NASA

Status of Theory
Dr. R L Bjork
RAND Corporation

Status of Experiments
Dr. Walter Herrmann
Dr. Arfon Jones
Massachusetts Institute of Technology

Projection Techniques
Mr. John Gurtis
Mr. J W Gehring
General Motors Corporation

Tuesday, 18 September 1962

0900 TECHNICAL SESSION IV

FRACTURE PHENOMENA

Introduction - Session Chairman
Dr. H J Plass
University of Texas

Static Fracture
Dr. George Irwin
Naval Research Laboratory

Time Dependent Fracture
Dr. C C Hsiao
University of Minnesota

COFFEE BREAK

Spall Fracture
Mr. C D Lundergan
Sandia Corporation

1100-1230 LUNCH

1230 SESSION V

Summary of Each Session,
Future Research Objectives,
and Discussion of National
Program.

Symposia Director for ASD

Dr. John E Keto

Symposium Chairman

Major General R G Ruegg

Symposium Deputy Chairman

Colonel L R Standifer

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GENERAL INFORMATION

REGISTRATION 16 September 1962

Registration will be held in the Main Lobby of the Dayton Biltmore Hotel on Sunday 16 September 1962 from 8:00 to 10:00 P. M. Late registration can be accomplished from 8:00 to 9:00 A. M. on 17 September 1962 at the Information Booth outside the Junior Ballroom of the Hotel. All attendees are required to register. The registration fee will be $2.00.

Each attendee will be provided with a personal identification badge certifying admission to all sessions of the Symposium. Badges will be valid only for the Symposium and need not be returned. Identification badges should be worn at all times during Symposium events.

TRANSPORTATION

Transportation will be furnished each day at 8:00 A. M from the VOQ. The return schedule will be announced and posted in the Hotel Lobby.

PUBLICATION OF SYMPOSIUM PROCEEDINGS

A report of the Symposium proceedings will be mailed to each attendee at his registered mailing address.

MESSAGE AND TELEPHONE FACILITIES

The message center is located outside the main entrance of the Junior Ballroom. Incoming messages will be taken by Symposium aides. Assistance will be given in placing telephone calls and sending messages. Individuals will be paged during Symposium sessions only in the event of an emergency.

Attendees may be reached during Symposium sessions by calling the Symposium Information Desk at 223-[illegible].

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